Vol. 41, No. 5 September, 1934

PSYCHOLOGICAL REVIEW PUBLICATIONS

Psychological Review

CONTENTS

PUBLISHED BI-MONTHLY
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION
BY THE PSYCHOLOGICAL REVIEW COMPANY
PRINCE AND LEMON STS., LANCASTER, PA.
AND PRINCETON, N. J.

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879

PUBLICATIONS

OF THE

AMERICAN PSYCHOLOGICAL ASSOCIATION

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Subscriptions, orders, and business communications should be sent to the

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, NEW JERSEY

VOL. 41, NO. 5 SEPTEMBER, 1934

THE PSYCHOLOGICAL REVIEW

PRIMITIVE FORMS OF BELIEF AND KNOWLEDGE

BY EDWARD L. THORNDIKE

Teachers College, Columbia University

In the course of experiments on learning which have been carried on during the past six years at Teachers College, I have had the opportunity to observe the natural history of belief, certitude and what the common man calls knowledge, in certain very simple cases. It is the purpose of this paper to report these observations and the conclusion to which they lead.

The cases concern learning which of a number of responses to a situation is the 'right' one, the one to be cherished and used, the one which the person comes to believe in or know, as he believes or knows that 9 plus 8 equals 17, or that Columbus discovered America in 1492, or that the name of the first president should be spelled W a s h i n g t o n .

Consider first instances of such learning where the 'right' or 'true' response is so by arbitrary edict, as when each of forty words has as its right response some number from 1 to 10, and where the learner hears a word, chooses a number, is told 'Right' or 'Wrong,' hears another word, chooses a number, is told 'Right' or 'Wrong' and so on through the forty words, and then through all forty again, and then again and again, until five or ten trials with each word have been made and recorded. At the first trial with any word the learner's response is determined by mere guess-work plus minor tendencies which he may have to favor certain numbers in general, to have one number rather than another

be brought to mind by a certain word, to avoid saying the same number many times in succession, or to favor doing just this. At the first trial he never or almost never has any belief that his response will be right. It will in the long run be right one time in ten, but there is normally no belief or fraction of belief attached to his first responses as a result of this. His belief in them is lacking equally and in the same way whether there is one chance in six, or one in eight, or one in ten, or one in fifteen, or only one in twenty.[1]

The learner then normally makes his first response with zero belief that it is 'Right,' or that it is 'Wrong.' When about two minutes later (about 3 seconds were allowed for the experimenter to say the word, the learner to choose a number, and the experimenter to say the next word) he has a second trial, he still usually has no belief that his response is 'Right' or that it is 'Wrong'. He does have very much stronger tendencies to say the numbers which were 'rewarded' by the experimenter's announcement of 'Right' in response to their respective situations than he would have had by chance, and than he has to say the numbers which were 'punished' by the announcement of 'Wrong.' In a pragmatic way his neurones are disposed as they were not before, but he usually has no more surety, 'hunches,' beliefs, or other feelings or thoughts different from what he had at trial one. In the third trial the inner history is much the same, with one exception. The learner is now aware that numbers occasionally come to his mind rather fluently and emphatically. Whereas in trial 1 his attitude was that of "say any number from 1 to 10" and he had to force himself somewhat to say any, he now finds that a word heard tends occasionally to evoke the thought of a number. He usually says that number, for want of anything more promising to do, and to save trouble. Such numbers turn out to be right much oftener than chance. The learner finds to his surprise that what still seem to him to be mere guesses, in whose correctness

[1] Except, as stated above, for rare and abnormal cases, as when, having tried some one number many times in succession with a uniform result of 'Wrong,' he believes it must be 'Right' the next time.

he still has hardly more confidence than at the start, are right far oftener than chance alone would cause.

In the meantime connections which were rewarded by 'Right' in both trials 1 and trial 2, have become strengthened far above their original probability of one in ten, and connections which were rewarded in either trial 1 or trial 2 have been somewhat strengthened. If the learner happens in trial 3 to respond to a word by the number which he has already used twice and been twice rewarded for using, that word → number connection will often be very strong when the word is heard in trial 4.

Rarely in the second or third trial, but increasingly often in the fourth and later trials a word will thus evoke a certain number with great promptness, emphasis and exclusiveness, and with a certain expectation or belief that it is right. Let us observe these nascent expectations or beliefs. At this stage there usually is no memory whatsoever associated with them of having said that number for that word before. There usually is no memory or image of the word 'Right,' as said by the experimenter (*i.e.* with his pronunciation, enunciation, or quality of voice), nor indeed of the word 'Right' in any form. There is usually nothing observable save a strong tendency to think of the number and say it, and a more or less strong expectation or belief that it will be called 'Right.'

With further trials the oft-rewarded tendencies manifest themselves in still greater promptness, emphasis, and exclusiveness of the idea of the number, and in an expectation or belief that reaches certitude. And now the subject may remember that he said that number before with success, or may think "6 has been right for this word repeatedly," or the like. But he may not, and often will not, and any contribution to his certitude by such memories or thoughts in these experiments is probably very slight. They come most frequently after certitude has been reached or nearly reached, and seem to be rather ineffective accompaniments of it.

In such experiences, having the form Sit → Resp → Reward (the occurrence of the connection and of an after-effect

in the nature of a reward belonging to it), there is a pronounced strengthening of the S → R connection, *i.e.*, an increase of the probability that the S in question will evoke the R in question. This occurs almost universally and builds up the correct habit rapidly. There may be also a formation and strengthening of a connection between the situation and an image or memory or idea of the response, and also an image or memory or idea of the after-effect. Such a connection seems to be much less universal and to gain strength much less rapidly. A subject may show demonstrable strengthening of the connection between the situation and the actual R, without having, so far as he knows, any tendency to have an image or memory of that R or of its after-effect. This is the rule in early trials and may be the case after he has reached a stage of strong expectation or belief. Expectation that a certain R will be satisfactory or belief that a certain R is right may be caused by the strength of the tendency for the S to evoke that R, without any inner representation of that R and its after-effect. This may sound like nonsense to readers who are accustomed to regard actions, choices, and attitudes as the consequences of images and ideas. "How," they will say, "can a person *expect* that a number will be right save by thinking of it and of 'Right' in conjunction? What can be meant by a *belief* that *4* is right for *water* save an attitude toward some sort of representation of *water* and *4*?" The answer is that he can expect that what he is saying will be right in the sense that he confidently says it and will be greatly surprised to hear 'Wrong,' without any such inner representations. He can be disposed to say 'Yes' to anybody who should ask "Do you really believe that 4 is right for water?" without any such inner representations. Such actions and attitudes on his part are evidence of the existence of something which may reasonably be called the basic, primary stuff of the expectations and beliefs that occur later in these experiments.

This basic primary stuff leads individuals A, B, C and D who have *no such* representations to do what E and F do *with such* representations. It leads individuals G, H, I and J

to do the same at a stage before they have such representations as they do at a later stage when they do have them. But there is no need to argue about names. Anybody who cares to deny the terms *expectation* and *belief* to the primary basic attitudes which I have described until they have images or memories or ideas of the expected or believed-in condition or event attached to them, may do so, and choose whatever names are fitting for them in their earlier naked condition. The fact of importance is that when a connection of a certain sort (here a connection between a word and a number made in an effort to attain the after-effect 'Right') becomes very strong by attaining that after-effect repeatedly, there develops along with the operation of such a strengthened connection the attitudes described, independent of and before any representations of the response or the after-effect.

There is a fundamentally identical quality or characteristic that develops in or becomes attached to any connection which has been strengthened to a high degree by the confirming reaction, whether the connection be one of motor skill, ordinary action, conduct, or thought. A connection so strengthened operates with no sense of doubt, impropriety or possible failure. Whether a person is writing his name, or is making a sure putt at golf, or is putting a spoonful of oatmeal into his mouth, or is paying a bill, or is thinking that 19 + 7 equals 26—the connection, which has been made strong by the confirming reaction in the past, has this quality or characteristic. Only in the last sort of cases do we say that a person 'believes' or 'knows' that — is —. I conclude that this is simply a verbal expression of the existence of this quality or characteristic in the special cases where the responses are of special sorts, notably ideas. If, in our experiments, the learners had tapped one of ten different keys or pulled a lever with ten different amounts of force instead of saying one of ten different numbers, nothing would have been altered fundamentally, but they would then usually not have thought or reported that they 'believed' or 'knew' that the degree of force used was 'Right,' though they might have done so. It is possible, and more or less useful, to say

"I knew that I was hitting the ball in," or "I knew that I would get the food into my mouth," or "I knew that I was doing what I wished to the ball."

The important matters in all connections are the fact of ability or competence, and the feeling of optimism or confidence (or the absence of contrary feelings) which accompanies it.

I do not maintain that the inner or conscious states of knowledge and confidence are caused by and accompany any sufficiently strong connections. On the contrary, I have limited the claim narrowly to strength as acquired by the confirming reaction. Whether certain other sorts of strengthening do or do not produce the attitudes of expectation, belief and knowledge, I shall not try to decide, but I conclude that not all sorts of strengthening do. Certain sorts of connections that are strong, but have not been confirmed by their after-effects as appropriate in view of the total set or purpose of the creature at the time, may inspire no confidence. Obviously, a person who is learning that *John* has *7* as its 'right' response and who has a 2-year-old son named *John* may have a strong tendency to think *2* when he hears *John*, without thereby being made confident that 2 is right in the experiment. In trial 1 he will have as little confidence in *John* → *2* as in *John* → *4* or *John* → *6* or *John* → *7*. What confidence he feels in *John* → *2* will be in *the age of my child John* → *2*. This, of course, has countless confirming reactions back of it. Somewhat less obviously, very strong connections, as to sneeze at a certain irritation of the membranes, or to laugh when tickled, or to be startled by an unexpected loud sound, may operate with no confidence.[2] Still less obviously, a connection may be very strong, but the person in question may be actually surprised to find it occurring. For example absorption in reading may connect strongly with biting the nails, yet the person as he becomes absorbed in reading may have no belief or expectation that he will bite his nails.

It will be profitable to consider the influence of negative

[2] In such strong unlearned connections there is also no doubt or sense of possible failure.

after-effects, consequences of a connection which reject it as useless or harmful to the ruling purpose of the creature at the time. Such might naturally be supposed to do one of two things. (1) They might cause an expectation or belief that the said connection is 'wrong' that the act is 'not to be made,' the idea is 'not to be accepted or expressed' and the like. Or (2) they might intensify the sense of insecurity, doubt, and possibility of failure. The former is the view of commonsense and probably of most psychologists; and in certain cases under certain conditions they do have this result. The latter could also happen and perhaps occasionally does. But our experiments lead me to conclude that negative after effects do not intrinsically and necessarily cause either of these results.

In our experiments the general rule is the amazing one that the announcement that a connection is wrong does not make the subject expect or believe *any differently from the way he did before*. In these experiments the situation vanishes and does not recur until after, say, forty intervening connections. When it does recur, the subject usually manifests the old lack of any belief or expectation, except in so far as previous satisfying after-effects have strengthened some connection by the confirming reaction. At a late stage, of course, the subject does come to know that x is wrong as the response to *so and so*, but usually this knowledge is an indirect inference from his knowledge that a certain other response is right. He may then believe it as firmly of a response which has never been called wrong as for one which has been called wrong several times.

The development of a belief that response 8 (of responses 1, 2, 3, $\cdots$, n) is right due to the 'positive' after-effects which confirm it occurs with no comparable belief that responses 2, 5, or 9 are wrong because of their 'negative' after-effects, and with no demonstrable change in the doubt felt concerning these latter responses as the subject makes them.

The unsatisfactory or negative after-effects, of course, inform the subject in each case that the connection which has just occurred is unsatisfactory to his purpose, and if

beliefs and expectations were primarily a function of the number of informatory occurrences, negative after-effects would often establish beliefs that certain connections were wrong. But they are a function, at least in part, of the number of confirming reactions. So, whereas four successive occurrences of a 'right' connection very often establish a considerable belief in its rightness, four successive occurrences of a comparable 'wrong' connection much less often establish a belief in its wrongness. If they did often establish a belief in its wrongness, the learner would be guided by this and would rarely make the same error a fifth time. Actually the probabilities of repetition of the same wrong connection in relation to the number of times it has occurred already are as follows in typical experiments:

One occurrence punished in trial 1, probability of recurrence in trial 2, .39
Two occurrences punished in trials 1 and 2, probability of recurrence in trial 2, .53
Three occurrences punished in trials 1, 2 and 3, probability of recurrence in trial 2, .67
Four occurrences punished in trials 1, 2, 3 and 4, probability of recurrence in trial 2, .69
(Average of two sets of Tuckman's data on code learning. Mere chance would give .25.)

One occurrence punished in trial 1, probability of recurrence in trial 2, .27
Two occurrences punished in trials 1 and 2, probability of recurrence in trial 3, .34
Three occurrences punished in trials 1, 2 and 3, probability of recurrence in trial 4, .36
Four occurrences punished in trials 1, 2, 3 and 4, probability of recurrence in trial 5, .41
(Average of two sets of Tuckman's data on learning nonsense forms.
Mere chance would give .16 2/3.)

One occurrence punished in trial 1, probability of recurrence in trial 2, .34
Two occurrences punished in trials 1 and 2, probability of recurrence in trial 3, .44
Three occurrences punished in trials 1, 2 and 3, probability of recurrence in trial 4, .49
Four occurrences punished in trials 1, 2, 3 and 4, probability of recurrence in trial 5, .52
(Average of selections from Lorge's data on code learning and learning the
meaning of Spanish words. Mere chance would give about .24.)

One does have occasionally an expectation or dread that whatever one says will be wrong; and this may be due to the relics of past negative after-effects to the situation at hand. I think it comes oftener, however, from a succession of negative after-effects to *different* situations in the immediate past. One does have, of course, occasionally a definite memory of having done thus and so with unsatisfactory consequences, causing occasionally a real belief in the wrong-

ness of that response, corresponding to a belief in rightness similarly caused. Both events are rare in our experiments.

The evidence that negative after-effects need not intensify insecurity or doubt is simply that the learner is unaware of any such intensification. Since the probability of such an intensification is not of general interest, no more need be said about it.

In these experiments a person learns to think and act well, at the start with no paraphernalia of beliefs or expectations, later with certain strong tendencies free from doubt or hesitation, and later still with little more than still stronger tendencies plus varying degrees of certitude that all is well in a given case. When the connection concerns ideas, the degrees of certitude may be called belief and knowledge. In the life of sophisticated human beings this early primitive confidence may be checked and supported by thoughts or memories of evidence in great elaborateness. It, or a good substitute for it, may be attained by *a priori* reasoning. It may be inspected and rated in grades from absolute certainty down, as when a man will bet 1000 to 1, or 100 to 1, or 10 to 1, etc., that his idea is right, or that his act will succeed. Its primary causation, however, does not seem to be by semi-logical cerebral summation of memories and evidence, much less of reasons and arguments. Nor, on the other hand, is it by a mechanical cerebral summation of occurrences and after-effects, in which an occurrence adds so much to the belief that X will occur, a reward adds so much to the belief that X is the thing to do, and a punishment adds so much to the belief that it is not the thing to do, or that something other than it is the thing to do. One feature of its primary causation seems to be that it is a by-product of the action of the confirming reaction upon connections. This action seems not only to make a connection stronger but also, after a certain strength is reached, to attach an attitude of security or optimism to the connection.

[MS. received March 29, 1934]

GREEK CONTRIBUTIONS TO THE TERMINOLOGY OF PSYCHOLOGY

BY O. W. REINMUTH

University of Nebraska

"The Greeks," they say, "had a word for it." In fact they had a word for more than just 'it' and out of their abundance they have made a larger contribution to our English language than any other language with the exception of Latin. Such common words as school, desk, idea, pause, paper, ink, story, poem, atlas, horizon, ocean, and chair have been ultimately borrowed from the Greek, although we have taken them, for the most part, at second-hand from the Latin. The stock of such every-day words which we owe to the Greek language is larger than we might suppose, as is evident from an amusing article of some five pages in which every single word with the exception of the articles and prepositions and certain other small words is of Greek origin.[1]

But these borrowings are insignificant in comparison with the overwhelming influx of modern scientific and technical terms which have been formed from Greek roots. The English language, which for the most part, has been a composite language, has coined or taken over from the Greek upwards of a million words, conservatively estimated, which were needed to express the ever-expanding knowledge in the different fields of scientific investigation, and this number is constantly increasing, particularly in the biological and medical sciences. Of more than 150,000 names of zoological genera, four-fifths contain some Greek element, and over 400,000 names of chemical substances are wholly or partially constructed from the Greek.[2]

In the field of psychology the proportion of words of Greek origin is not so large as it is in many others. At a rough

[1] F. P. Donnelly, Is the ostracism of Greek practicable? *America*, March 15, 1919.

[2] J. C. Smock, The Greek element in English words, New York, 1931.

estimate, about one-third of all the terms occurring in such a small hand-book as Giese, 'Psychologisches Wörterbuch,' fall in this class, but the proportion would be somewhat larger in the fuller and more technical lists.[3]

The formation of technical vocabularies from non-native root words has several things to commend it. Such words are generally understood the world over, and since they are constructed to designate a definite concept and have no other use, they have the precision of a mathematical formula. It seems to me that the technical vocabulary of no other field suffers so much from the fact that its terminology is in great part made up of words which are current in every-day use with a wide range of meanings, but which assume a specialized meaning within the field, as occurs in the field of psychology. A few examples of such words from a much longer list are—correlation, response, stimulus, suggestion, association, conditioned, reaction, personality, experience, distribution, concentration, attention, functional, sensation, reflex, and faculty. If these concepts were expressed in terms taken from the Greek, they could be used by scholars of all nations without change in form or meaning, but since they are Anglicized Latin words with literary as well as technical meanings, each language must use a native word with its own range of applications. Much would be gained in system and definiteness if psychologists would follow the practical rule adhered to in the biological and physical sciences that the basis of scientific nomenclature shall be classical Greek.

It is not a fortuitous circumstance that our scientific terminology is so largely Greek. The Greeks were beyond all other peoples interested in the various fields of human thought, and in very many instances were the first to organize and develop the different departments of knowledge. This

[3] H. B. English, A student's dictionary of psychological terms, Yellow Springs, Ohio, 1928, is a work in English comparable to Giese. F. Giese, Psychologisches Wörterbuch, 2 Aufl., Leipzig, Berlin, 1928; J. M. Baldwin, Dictionary of philosophy and psychology, 3 vols., New York, 1901–05, is antiquated. The editor informs me that a comprehensive work, Dictionary of Psychology, edited by H. C. Warren, is about to be published by Houghton Mifflin Company, Boston, Mass.

would be indicated by the fact that the Greek has supplied the name of almost every one of them, even if we did not still possess other more definite evidences of the researches of that gifted people in these fields. Psychology, Mathematics, Philosophy, Economics, Biology, Zoology, Botany, Rhetoric, Poetry, Philology, Politics, History, Chemistry, Geography, Grammar, Music, Architecture, Physiology, Anthropology, Medicine, Anatomy, Geology, Theology, Astronomy, and Archæology are a few of many examples. Since we are following the same lines of investigation, it is a waste not to adopt terms that have already been formed and have received a more or less precise meaning within a given field.

As in playing cards a relatively small number give rise to an almost infinite number of combinations, so a few Greek roots may run through almost endless permutations. The Greek language is peculiarly adapted to the formation of compound words so that even complex concepts may be expressed by a single linguistic unit which, although it may be cumbersome for every-day currency, is economical, precise, and convenient in the expression of scientific ideas.

Borrowed Greek roots have usually been limited to a single precise meaning, although their range of connotations in the original is much wider. Thus the root *psyche* originally meant 'life' or 'soul' as contrasted with body and spirit, but in our English derivatives it is applied specifically to the mental activities excluding the physical and physiological. The familiar ending, *-logy*, is often erroneously referred to the root *logos*, 'word,' 'account,' or 'science,' but it is rather to be derived from the abstract noun formed from *lego*, "I speak." Hence psychology has the etymological significance, 'a discourse about the mental processes.' Once we know the meaning of *psyche*, the clue to a large number of words is in our hands. *Psyche* combined with *iatreia*, 'healing,' gives us psychiatry, 'the healing of mental diseases' (*cf.* pediatrics, in which the root 'healing' is combined with *pais*, *paidos*, 'child'; hence, 'the treatment of children's diseases'); combined with *metron*, 'measure' we have psychometry, 'the measurement of mental processes'; with

genesis, which has been taken over root and branch from the Greek, 'beginning,' psychogenesis, 'the beginning and development of mental life.' Psychography is literally 'soul writing' or 'recording' and pertains either to a description of the mental processes, or more narrowly to spirit writings; and psychograph is the instrument to record such writings. Psychic gets its meaning 'occult,' from its limitation to the unusual operations of the mind, such as telepathy (itself a Greek derivative from *tele*, 'at a distance,' and *pathos*, 'feeling'; hence, 'the reception of sensation at a distance from the stimulus'), spiritism, and hypnotism, which word from its Greek roots simply means 'the action of putting to sleep.' The psychoanalyst to be true to the meaning of his name must 'break up' that is 'analyze' mental processes.

The divisions and schools of psychology are legion. Approached from the purely philosophical viewpoint we may call our field of study psychosophy, 'mind wisdom,' or the metaphysics of the mind. Metaphysics was the term applied by one of Aristotle's early editors to the writings of Aristotle which followed his works on *physis*, 'nature,' *meta*, 'after,' and *physica*, 'the things pertaining to nature.' The word was understood by the scholastic philosophers to mean 'the things beyond nature,' hence 'transcendental' and this interpretation prevailed. Looking at our field from the scientific point of view we have empirical psychology. Empirical is made up of *en*, 'in' or 'upon,' *peira*, 'a trial,' and the suffix *icos*, 'pertaining to.' Pirates and empiricists have this in common—they place great emphasis upon trial or attack. Mechanistic psychology would make our mental activities explicable by the laws governing the machine, while the hormic theory, *hormao*, 'urge' or 'stimulate' (*cf.* hormone, a bodily secretion stimulating or activating certain glands and physiological processes) maintains that behaviour is characterized by an urge toward a goal and is therefore purposive. Dynamic psychology deals with the *dynamis* or 'force' which brings about changes in mental processes, while a dynamometer measures, and a dynamograph records muscular *dynamis*. In its civic aspects, psychology is interested

that men be born well, eugenics, and be placed well, euthenics, that is placed in a favorable environment; while educational psychology is diagnostic, *dia*, 'between,' *gnosis*, 'discrimination' or 'knowledge'; *i.e.* it attempts to discriminate among the mental processes, and prognostic, *i.e.* it attempts to gain knowledge of the probable development of mental life in advance, and finally pedagogic, *i.e.* educational. The pedagogue, *pais*, *paidos*, 'boy,' and *ago*, 'lead' was originally the slave that led the noble lad to school, carrying his books and wraps, and in general acting as his valet; later the word was applied to the tutor or teacher. Psychopathology concerns itself with the diseases, *pathos*, 'suffering,' 'disease' of the mind while psychotherapeutics treats of the curative remedies applied to diseases of the mind. Therapy is derived from the Greek word meaning 'a remedy' and occurs in compounds like hydrotherapy, which is a system of remedial treatments involving the use of water as electrotherapy involves electricity and thermotherapy, the curative application of heat.

The present participle of the verb 'be' in Greek is *on* and the genitive case which invariably shows the full root of any Greek word is *ontos;* ontology, therefore, means 'the discourse about being,' ontogeny, 'the origin of being' or 'individuality' and ontogenetic psychology is the discourse about mind activity as it originates and develops in the individual, and phylogenetic is a similar treatment for the *phyle* or 'race.' The participle of another verb, *phainomai* 'appear' has passed over into English in the neuter form phenomenon with its original Greek meaning, 'the thing which appears.' The Greeks themselves used this word to denote any observable fact or event in its changing aspects as distinguished from its permanent essence. The phenomenal world was thus contrasted with the ontal world. In much the same sense psychologists use the word phenomenalistic to refer to the theory that there is no existence except in that which can be observed. Because phenomenon was thus contrasted with reality, the word phantasy, from the same root 'appear' came to refer to the unreal, that is the imaginative, although it really means, 'that which is seen,' and by frequent use assumed a simplified spelling, 'fancy.'

Anecdotalism is employed to characterize a highly amateurish study of psychological phenomena which rests upon the reports of casually observed incidents. The word is made up of *an*, 'not,' *ek*, 'out,' and *dotos*, 'given,' and should refer to an incident or story never told before, but alas, most anecdotes are so hoary with age that Adam may well have told them. But it is an idiosyncrasy of most morons, about whom psychologists speak some times, to find delight in anecdotes. In idiosyncrasy we have three Greek roots, *idios*, 'one's own,' 'peculiar to one's self' (an idiot is unique in his way, not only etymologically, and an idiom is 'a turn of language characteristic of one language') and *syncrasis*, 'a mixing together,' which gives us the concept, 'a peculiarity of one's mixture or constitution.' *Moron* is the Greek word for 'fool' and differs from idiot in being more general and less peculiar. A sophomore is 'a wise fool' and the word itself appears in a figure of speech which we have called oxymoron, *oxys*, 'sharp,' and *moron*, 'foolish,' that is 'pointedly foolish' said of combinations that are apparently inconsistent, as for example, 'cruel kindness,' 'falsely true.'

The field of abnormal psychology is particularly rich in words taken from the Greek. One never really appreciates the number of 'phobias' that harass the human race until the possibilities are investigated. Phobia is derived from the Greek word *phobos*, 'fear.' That hydrophobia is characterized by a morbid dread of water, we know. Agoraphobia is the dread of being in open places, *agora*, 'the market place.' The roots, *acros*, 'peak,' and *hypsos*, 'high,' combined with *phobia* designate the abnormal fear of high places, while bathophobia, *bathos*, 'depth,' represents the feeling of dizziness that is accentuated in certain individuals upon looking up at high buildings. Phagophobia is 'the dread of swallowing' exhibited in certain children especially when it is time for them to take a dose of castor oil, while toxiphobia refers to the fear that the guests of the Borgias may well have felt. It is interesting to note that *toxicon* in the Greek is the adjective meaning 'pertaining to the bow,' and came to designate poison because the tips of the arrows were commonly

smeared with poison which was called *toxicon pharmakon* but the word from which we get our pharmacy and which meant 'poison,' was usually omitted just as we call for ham on rye without supplying 'bread,' and *toxicon* meant only 'poison' and had no reference to a bow. Other common *phobias* are carcinophobia, 'fear of cancer,' monophobia, 'of being alone,' lalophobia, 'of speaking,' which is essentially present in those who stutter, staso- and basophobia, conditions in which the patient does not feel capable of 'standing,' *stasis*, or 'walking,' *basis*. Erythrophobia is most common among timid girls and retiring men, 'an excessive fear of blushing,' *erythros*, 'red.'

Similarly words denoting an abnormal liking, end in *philia*, which means 'love.' In claustrophilia we have an example of word formation to be avoided—*claustrum* from the Latin, 'a bolt' or 'bar,' joined with the Greek root *philia*, 'love.' Such a word is called a hybrid and is undesirable because it leads to confusion and lack of uniformity. Pædophilia, 'erotic passion for children,' and necrophilia, 'love for dead bodies,' are other examples.

The termination *-mania* is the transliteration of the Greek word meaning 'a rage, craze, madness.' Megalomania in plain corn-bread English is 'big-headedness.' The person thus afflicted has an insane craze for doing great and grand things. *Megalo* is the combining form derived from *megas* meaning 'great' which we have in megaphone, megacephaly, megatherium. Kleptomania, *klepto*, 'steal,' is about as common as metromania, *metron*, 'measure,' a mania for writing verse. Monomania from *monos*, 'alone,' is the derangement of the mind in regard to a single subject, as for example hydromania, 'the insane desire to commit suicide by drowning,' and pyromania, 'the craze to kindle a fire,' and nymphomania, *nymphe*, 'bride,' the morbid sexual craze of a woman for the opposite sex, which is one form of erotomania, *eros*, 'love' and personified, 'the god of love,' the Greek Cupid. From *eros* we get *erastes* 'the one who loves' which occurs in pæderasty.

A few simple prefixes find frequent application. The

letter *a*, which becomes *an* before vowels, negatives the meaning of the root to which it is prefixed. Thus amnesia, means 'not,' *mnesia*, 'remembering'; hence, 'forgetfulness,' and an amnesty in common usage is a 'forgetting,' or 'overlooking,' and thus 'a pardoning.' Analgesia has for its second root, *algia*, 'pain,' which is found in many words, neuralgia, nostalgia, and denotes 'insensibility to pain.' In the same way, anopsia means 'visual insensitivity,' anorexia, 'lack of appetite,' anesthesia, 'loss of sensitivity,' anosmia, 'insensitivity to smell,' asemia or asymbolia, 'loss of power to understand signs' (*cf.* semaphore, for *sema*, 'sign'), aphasia, 'loss of speech,' and our common term, apathy, from *pathos*, 'feeling,' 'lack of feeling or interest.' Aprosexia comes from *a*, 'not,' *pros*, 'to,' and *echo*, 'hold' and denotes the rather widespread failing of not being able to hold one's attention to a thing. Absence of muscular coördination is called ataxia, *a*, 'without,' and *taxis*, 'arrangement, system.' The prefix *dys* means 'hard,' 'bad' or 'ill,' and in psychological terminology may usually be interpreted, 'impairment.' Dysacousia is a condition in which ordinary sounds cause discomfort and is connected with an impairment of the sense of hearing. That feeling of dissatisfaction and restlessness which we commonly refer to as the 'fidgets' is called dysphoria, from *dys*, 'hard,' and *phero*, 'bear.' Dysgenic, containing the root *gen*, 'birth' or 'offspring,' refers to anything that is 'hard on' the innate quality of the offspring. A nervous affliction sometimes shows itself in the difficulty of joining the syllables of words together in utterance, which is called dysarthria, 'hard,' *arthron*, 'joint.' The general term for any impairment of the speech faculty is dysphasia. Since *lalia* comes from *laleo*, 'speak' (*cf.* German, "dann werd' ich nicht mehr Lieder *lallen*"), dyslalia means almost the same thing as dysphasia but it is usually restricted to the sense of dysarthria. The root *lalia*, is found in glossolalia, *glossa*, 'tongue,' and designates a manifestation of theomania, or religious ecstasy known as 'speaking with tongues,' 'uttering strange sounds.' Incidentally the word ecstasy is from a Greek root *ek*, 'out of,' and *stasis*, 'standing,' and has the same significance as our

English idiom, 'to be beside oneself' (with fear or the like). The Greeks said to stand *outside* oneself, we say to be *beside* oneself.

Para, or simply *par* before roots beginning with a vowel, means 'alongside,' 'beyond,' 'against,' 'amiss,' and hence 'irregular' or 'unusual,' or 'false.' The simple meaning is seen in the word 'parallel,' from 'alongside,' and *allelon*, 'each other' or parable, literally, 'a placing beside.' One whose mind is a little 'off' or irregular suffers from paranoia and is called a paranoiac, *para*, 'irregular,' 'amiss,' and *nous*, 'mind.' Parapsychology deals with irregular mental phenomena. A paradox, *doxa*, 'opinion,' is something that is 'against popular opinion,' paracousia, 'a derangement of hearing,' paralalia, 'a derangement of speech,' paraphonia, 'a derangement of the voice,' while paralgesia represents the morbid condition which misinterprets pain as pleasure.

To describe one who is all wrapped up in himself and in an imaginative world of his own we use the word autistic, from *autos*, 'self,' and suffix *ic*, 'pertaining to,' or we call him by the word from the Latin, an introvert, literally, "one who turns inward." Autism is often an indication of schizophrenia, formerly called *dementia præcox*. Schizophrenia literally means 'split personality' (*cf.* schism, 'a split in the church,' schist, 'a split in rock,' and scissors). The prefix *auto*, 'self,' is found in autoeroticism, 'the arousal of sex feeling by one's own acts,' autohypnosis and autokinetic, *kinesis*, 'movement,' 'the apparent movement of a point of light in a dark room.' The root *kineo*, 'move,' is found in kinetic and in our familiar cinema. Another form of *dementia præcox* is hebephrenia which is characterized by a childish behavior in adults. *Hebe* is the Greek word for youth, which personified becomes Hebe, the goddess of youth and the cupbearer to the gods.

The preposition *hyper* has the meaning, 'beyond,' and prefixed to a word it signifies 'high degree' or 'excess.' Hyperesthesia is 'sensitiveness beyond the ordinary,' hypermnesia, 'unusual ability to remember,' hyperalgesia, 'supersensitiveness to pain.'

The prefix *hypo* has just the opposite significance, 'under,' 'defective.' Hypochondria is from the Greek words meaning 'under the cartilage of the breast bone' where was thought to be the seat of feeling. In hyperopia, the light rays from objects converge *beyond* the retina instead of directly upon it, while myopia, from *myo*, 'close,' or 'contract,' refers to a condition in which the light rays come to a focus in front of the retina. Hemeralopia, from *hemera*, 'day,' means exactly what it says: 'sight in the daytime,' and nyctalopia refers to the ability common in cats of seeing well at night. Normal sight is expressed by the word emmetropia, which means literally *en*, 'in,' *metron*, 'measure,' or 'proportion,' and *op*, 'seeing,' hence 'seeing objects in their true proportions.' Abnormal vision, characterized by seeing uncolored objects as colored, is called chromopsia, *chroma*, 'color'; and more definitely cloropia, erythropia, xanthopia, as the objects are seen to be colored green, red, and yellow respectively. Many men are disqualified for work as engineers or aircraft pilots because of their inability to distinguish red from green, a condition called deuteranopia, that is *deuteros*, 'second,' *an*, 'not,' and *op*, 'seeing,' hence, 'not able to distinguish the second of two colors (red and green).' In certain normal and abnormal conditions, as for example in the case of intoxication, men are subject to diplopia, *diplous*, 'double,' and *op*, 'sight,' that is, seeing double.

Instruments used in the psychological laboratory will more than likely be called by some name ending in *-graph*, from *grapho*, 'write,' or 'record,' or ending in *-scope*, from *skeptomai*, 'examine,' or 'observe' (*cf.* skeptic, "one who examines"). A cardiograph is an instrument to record the beat of the *kardia*, 'heart,' the sphygmograph records the *sphygmos* or 'pulse beat,' the ergograph records the force exerted in continuous work, *ergon*, as cinematograph records the movement of objects upon a screen. Dermatographia, *derma*, *dermatos*, 'skin,' on the other hand, denotes the condition in which a mark appears upon the skin when it is rubbed. A stroboscope is an instrument for demonstrating the so-called stroboscopic effects in which an object apparently

at rest seems to move, *strobeo*, 'spin,' or 'whirl.' A tachistoscope, *tachistos*, 'speediest,' and *skopos*, 'examination,' is an instrument for providing a very brief exposure of visual material which is to be memorized or closely observed. The names of other instruments end in the familiar *-meter*, Greek *metron*, 'measure.' Thus an haptometer, *hapto*, 'touch,' measures the sensitivity of touch, a thermoesthesiometer measures the perception of heat, a phonometer measures sound, and a photometer, light.

The Greek root from which narcotic is taken, *narkoo*, 'benumb,' occurs in a number of roots. Narcomania is the uncontrollable craze for those substances that bring on a *stupor*. Narcissism is the broader term including autoeroticism, which is applied to the abnormal love of one's own deeds and qualities and derives its names from the mythological Narcissus, who was deeply loved by the nymph, Echo. When Narcissus failed to return this love, Nemesis punished him by causing him to fall in love with his own reflection which he saw in the placid waters of a pool and for which he pined so deeply that he passed away and was changed into the flower that bears his name. The narcissus flower has narcotic properties. Narcolepsy contains in addition to the root *nark*, the root *leps* from *lab* meaning 'seize upon,' 'attack,' and is the name for a condition characterized by attacks of deep stupor. Epilepsy, *epi*, 'upon' signifies a 'seizure upon,' and catalepsy, *kata*, 'down,' an attack falling *down* upon one.

The Greek word *eidos*, 'form,' 'resemblance' gives us eidetic (image), that is an image of absent objects which is as vivid and complete in detail as the original image. *Eidos* appears as a terminal combining form, *oid*, and means 'having the form or likeness of,' asteroid, 'like an *aster*,' *aster*, 'star'; anthropoid, 'like an *anthropos*,' 'man,' spheroid, like a *sphaira*, 'ball.' The word 'idea' is a simple transliteration of the Greek word and like *eidos* it is derived ultimately from the root *id*, 'see.' Although it has been used in a loose way and in a wide variety of meanings even in technical philosophical language, *idea* is distinguished from *eidos* by

the fact that the former refers to the form or resemblance which is the product of our imaginative or purely mental powers, while the latter refers to an image which is perceived by the sense organs.

The Greek contributions to the psychologist's technical terminology are by no means limited to the words that may be found in a psychological dictionary. Psychology touches so intimately upon the fields of philosophy, physiology, medicine, biology, anthropology, and physics, to mention but a few of the most closely related departments of knowledge, that the technical vocabularies of these sciences must to a large extent be a part of the working vocabulary of every psychologist. The Greek element in these fields is already very large and further coinages from the same language as a matter of uniform practice are constantly being minted.

It is only comparatively recently that psychology has taken its place as a separate field of investigation among the sciences, and the nomenclature peculiar to it is still in the making. It is fortunate that this is true, for psychology may well profit by the experience of other sciences in securing for its technical vocabulary four desirable qualities, consistency, uniformity, permanence, and possibilities for new formations which are consistent with the old. The way to achieve these highly desirable qualities in the nomenclature of psychology is the path that is already being followed in the biological sciences—the use of Greek roots, and their combination in accordance with a few simple rules which the psychologist who wishes to frame a new word will find it worth his while to know.

[MS. received February 19, 1934]

ON EMOTIONAL EXPRESSION AFTER DECORTICATION WITH SOME REMARKS ON CERTAIN THEORETICAL VIEWS: PART II [1]

BY PHILIP BARD

Johns Hopkins School of Medicine

II. Modes of Emotional Expression Evocable in Surviving Decorticate Animals

As has already been pointed out[2] several workers (**12, 17, 34**) have reported that signs of anger can be elicited in dogs and cats during prolonged survival periods following decortication. These writers, however, were not particularly concerned with this aspect of behavior and did not devote a great deal of attention to it. Therefore, it has seemed worth while to reinvestigate the matter and to determine more precisely, if possible, the conditions under which a display of anger can be evoked in such animals. It seemed especially desirable to compare at first hand the chronic with the acute preparation and, as will be seen, it proved possible to prepare what amounted to a surviving hypothalamic cat in which responses closely approximating sham rage were obtainable. Furthermore it was thought that if a reaction pattern such as that of anger can be elicited after decortication, other forms of emotional expression may also be evocable. No indication that such is the case had been given in the earlier reports of such preparations and the matter was open for exploration.

Up to the present time (January, 1934) the writer has prepared and studied during long survival periods four cats and three dogs in which the least extensive cerebral ablation

[1] From the Laboratories of Physiology in the Harvard Medical School and the Department of Physiology, Johns Hopkins School of Medicine.

The work which is briefly described in this Part has been aided by a grant from the Macy Foundation. It will be reported at length in another publication.(Bard and Rioch).

[2] See Part I, Psychol. Rev. 1934, 41, 309–329.

has been bilateral removal of all neocortex. The brains of all these animals have been serially sectioned and in the case of the cats a full histological study has been carried out by Dr. D. McK. Rioch of the Department of Anatomy, Harvard Medical School.[3] The anatomical study of the dog brains has not yet been completed. Detailed reports of the general behavior of these seven animals together with descriptions of the neuro-anatomical findings will soon be forthcoming (Bard and Rioch; Pinkston, Bard and Rioch). Therefore, in the present account, no more space will be devoted to these aspects of the work than will serve to give the necessary background against which the emotional responses may be judged. The surgical methods employed have been described elsewhere (4) and need not be repeated here.

The decorticate cats.—The first animal of the decorticate series was cat 103, a small mature female that had been kept under observation for nine months before the first intracranial operation was performed. During that time she showed herself to be an alert, gentle, clean and friendly animal. She always purred when stroked and she submitted to rather rough handling without displaying anger or fear. She showed anger only when actually hurt. In contrast to her friendly attitude toward human beings she usually displayed resentment when approached by another cat of either sex. Early in this observation period (in November, 1928) a double ovariectomy was performed (but see page 444).

The first intracranial operation to which No. 103 was subjected was carried out July 5, 1929, and its objective was the removal of all neocortex of the right hemisphere. From this procedure she made a rapid and uneventful recovery. During the interval between the first and second cerebral ablations this cat exhibited precisely the same characteristics that had been noted during the observation period. In the light of an experience with some thirty unilaterally decorticate cats it can be said quite confidently that after removal of neocortex or all cortex of one hemisphere the individual

[3] To Dr. Rioch I am greatly indebted for the reports of his anatomical studies and for his generous coöperation in the physiological aspects of this work.

traits of a cat are not ostensibly altered. Ablation of the remaining (the left) neocortex was done December 23, 1929. After a rapid recovery from the immediate effects of the anesthesia (sodium barbital) her general behavior remained quite constant. Except for two or three periods of gastro-intestinal disturbances her health remained good up to the latter part of 1931 when she developed an ever increasing tendency to bite herself in response to the slightest cutaneous irritation. Because of the severity of some of the wounds thus inflicted she was killed in April, 1932, two years and four months after the second and final cerebral ablation. Dr. Rioch's study of the serial sections (Pal-Weigert) of her brain revealed the following facts:

Neocortex. This had been completely removed on both sides.

Rhinencephalon. The greater part of the olfactory bulbs and stalks had been left intact. The medial basal olfactory area remained on both sides and shows fiber connections with the olfactory bulbs. The pyriform lobe and amygdala are almost wholly intact on the right, but had been replaced by a large cyst on the left. The hippocampus had been removed on the left, and on the right there remained only a degenerated fragment of this structure.

Striatum. The parts spared were the caudomedial portion of both caudate nuclei, most of each putamen and all of the globus pallidus and entopeduncular nucleus on each side.

Thalamus. On each side the lateral portions of the dorsal thalami had been ablated, but the periventricular system and the medial portions of the anterior, medial and ventral groups of nuclei were intact. The optic nerves and chiasm had a normal appearance, and although the optic tracts did not seem reduced in size, both lateral geniculate bodies had been damaged laterally. The medial geniculates were normal. The habenular ganglia and the habenulopeduncular tracts remained.

Hypothalamus. This had not been damaged. The medial and lateral preoptic and hypothalamic areas were intact. Although the descending columns of the fornix were absent, and there was a reduction in the size of the bundle of Vicq d'Azyr, the fibers of which could not be followed into the anterior thalamus, the tegmental connections of this region were apparently normal.

Extrapyramidal system. The ansa lenticularis, fields of Forel,

nucleus subthalamicus, zona incerta, substantia nigra and red nucleus were bilaterally normal.

Mesencephalon and rhombencephalon. Except for degeneration of the peduncles and pyramids and a reduction in the size of the superior colliculi and the pontile nuclei, these divisions were unaffected by the ablations.

Throughout her long survival without neocortex cat 103 exhibited no visual responses other than brisk pupillo-constriction and blinking on illumination of the eyes. Auditory responses were easily obtained. A light whistling noise or the sound of sandpaper rubbing against wood or a sudden metallic click evoked elevation and pointing of the ears, widening of the palpebral spaces and turning of the head toward the source of the sound. This response was usually elicitable only after a period of quiet and when the animal was not restless; whenever it was repeatedly evoked it became less marked and soon disappeared. Correlated with the integrity of a large part of the rhinencephalon was a most definite response to certain odors. Most striking was the sniffing, licking and moving about with mouth close to the floor when a sapid food was placed nearby. After a hearty meal this behavior could not be evoked and it was most pronounced when food had been withheld. The postprandial chop and nose licking with dorsiflexion of head which is seen in normal cats was here exaggerated and prolonged. There was no ability to locate the source of odors. When a paw was wet or soiled it was often shaken, sometimes lifted and occasionally licked. Gentle rubbing of any part of the body surface except the head induced vigorous licking and when the stimulus was somewhat stronger (scratching) biting occurred. When the dorsal midline was thus stimulated these responses of tongue, mouth, and neck were directed forward and downward so that the cat licked and bit the floor or its forefeet. Advantage was sometimes taken of this reaction to speed or extend the ingestion of food. Usually, however, when food was placed under her nose she ate adequately of her own accord. When the cutaneous stimulus was to one side of the midline the head turned to that side, but only rarely did

the animal lick or bite the exact area stimulated. When mange was present this reaction was so intensified that she often bit her chest, shoulders or flanks and during the last weeks of her career, when many self-inflicted wounds provided a continuous source of irritation, slight cutaneous stimulation often evoked a generalized convulsion in which the animal tore viciously at its skin.

Specific deficiency phenomena were prominent. A recent paper (4) has described and analyzed the postural defects of this and other decorticate cats. Locomotion was good, but not quite normal (see 4). The occurrence of walking could usually be correlated with either hunger or a distended bladder or rectum. Although the chest and forelegs were licked spontaneously there was failure to keep the rest of the body surface clean. There is evidence that the typical feline care of the coat is dependent on cerebral cortex and specifically upon the sensorimotor area located at the frontal pole (4). This cat slept as do normal cats, but when fully asleep it required shaking, handling or a nociceptive stimulus to awaken her. Sometimes on waking this cat performed typical stretching movements.

Of the other decorticate cats No. 313 was the one which in postoperative behavior and the extent of the cerebral ablations most closely resembled cat 103. This was a female which at the time she entered the laboratory was excessively wild. During a preoperative observation period of five months she was always very timid, ran off whenever approached and made frantic attempts to escape when caught. It proved impossible to tame her, although she spontaneously became tractable for a short time following parturition. These very definite characteristics remained unaltered after the removal, on July 27, 1932, of all neocortex of the left hemisphere. After a rapid and uneventful recovery from a similar operation performed on the right side, October 15, 1932, this animal duplicated in her general behavior the conditions exhibited by 103 in the decorticate state. Cat 313, however, was anosmic. She remained in excellent health for six weeks and then suddenly succumbed to an acute respira-

tory infection. The results of histological study of serial sections of the brain may be summarized as follows:

Neocortex. Entirely removed except a small portion of area 32 in front of septum.

Rhinencephalon. Olfactory stalks had been cut through. Septal nuclei intact. Tuberculum injured only at right margin. Left pyriform lobe injured laterally, but on right only medial third remained. On right there was only a ventromedial fragment of hippocampus; on left the dorsocaudal portion was removed. Fornix destroyed bilaterally.

Striatum. On left intact except for lesion of dorsolateral portion of head of caudate and lateral margin of putamen. On right the ablation removed lateral part of head and all of tail of caudate, all of putamen and lateral portion of globus pallidus rostrally.

Thalamus. Not involved in ablation on left except for injury of lateral border of lateral geniculate, but in lateral nuclei there was extensive degeneration (gliosis and loss of ganglion cells). On the right the dorsolateral margin of lateral nucleus was removed and the lateral geniculate cut through. On right degeneration of nuclei was more marked than on the left.

The rest of the brain stem was intact and presented no changes except degeneration of pyramidal tracts, reduction in size of hypothalamus and anterior colliculi.

Cat 244 was subjected to a somewhat greater removal of cerebral tissue than were cats 103 and 313. This animal was kept for eleven months with the brain in the condition indicated in the following report of Dr. Rioch's study of the serial sections:

Neocortex. On either side there remained a small *disconnected* fragment of temporal cortex. The rest of the neocortex had been removed from the cranium.

Rhinencephalon. The olfactory stalks and septum had been removed. There remained on the right side the entire tuberculum olfactorium, the caudal end of the hippocampus and all of the pyriform lobe except its lateral margin. On the left only the medial part of the tuberculum and pyriform lobe and the tail of the hippocampus remained.

Striatum. On the left only a few cells of the globus pallidus and entopeduncular nucleus were present. On the right these structures

were intact and the basal caudal portion of the putamen and claustrum were present, but showed considerable gliosis.

Thalamus. Aside from the central gray, the n. parafascicularis, the centre médian, the n. suprageniculatus, n. posterior and caudal pole of n. lateralis, all of which showed gliosis, the thalamus proper had been removed. Only the ventromedial layers of the lateral geniculates were present. Both optic tracts appeared normal. The habenular ganglia were intact. The medial geniculate was injured on the left, intact on the right.

Hypothalamus. Intact on both sides with moderate reduction in size.

Extrapyramidal system. The zona incerta showed gliosis rostrolaterally. The cells of the n. subthalamici and substantia nigra were crowded. The red nuclei showed some gliosis and were reduced in size.

Mesencephalon. Cerebral peduncles had entirely degenerated. The reticular substance was reduced slightly in size and showed some gliosis. The rostral pole of the superior colliculus was injured on left and lamination was indistinct.

Rhombencephalon. No change except degeneration of pyramids and reduction in size of pons due to loss of fibers.

During a preoperative observation period of three weeks, this mature female cat proved to be a gentle, very friendly and alert animal. The operation on the right side (November 3, 1931) was followed by a rapid recovery. During the interval between the first and second operations this cat became pregnant, delivered four kittens and nursed them in normal fashion. The removal on the left side (July 14, 1932) was slightly more extensive. After an uneventful recovery the animal remained in good health until sacrificed July 16, 1933. Her general behavior differed only in a few respects from that of 103. Like cat 313 she lacked olfactory responses and she ate of her own accord less readily than either 103 or 313. A singular and very striking item of behavior appeared during the latter six months of the survival period. At times when the animal was allowed the freedom of a large laboratory room it was noticed that she followed one about. On several occasions she followed the writer out into a corridor, along it for ten to twenty yards and back

again into the room. At other times it was impossible to obtain this response and we were unable to determine the conditions facilitating it. That the adequate stimulus was auditory seems certain. When following was evocable her responses to sounds were particularly brisk and she would often turn and walk or even run toward the source of a light whistle or scraping noise. A similar phenomenon was observed by Dusser de Barenne (**12**) in the case of his cat II.

In the case of cat 228 (an elderly female) the surgical objective was the production of a hypothalamic animal. But in the course of each of the two operative procedures difficulties arose that made it hazardous to attempt this goal. Nevertheless, study of the serial sections has shown that very little tissue above this region remained. Briefly summarized, Dr. Rioch's findings are as follows:

Neocortex. Entirely removed except for small *disconnected* fragments of temporal cortex.

Rhinencephalon. The only parts spared were the n. accumbens and a small disconnected fragment of hippocampus on both sides, the ventrocaudal portion of the septum and a caudomedial portion of the tuberculum olfactorium on the right side.

Striatum. Entirely removed except possibly a few cells of the medial portion of globus pallidus on the left.

Thalamus. Only the most medial part of the dorsal thalamus remained and it showed considerable reduction in size, distortion and fiber degeneration. Both optic tracts had been cut and were degenerated. The lateral and medial geniculate bodies were ablated on both sides and von Gudden's commissure had degenerated. The habenular ganglia were intact and the medial and ateral habenulo-peduncular tracts appeared normal.

Hypothalamus. Entirely intact on left, but on the right the lateral preoptic area, the lateral half of the medial preoptic area and, rostrally, the lateral hypothalamic area had been removed.

Extrapyramidal system. On both sides the zona incerta was involved rostrolaterally in the ablation. The subthalamic nuclei remained on both sides but were abnormally small and showed very few fibers. The ansa lenticularis was absent on both sides and the fields H2 of Forel were almost devoid of fibers. There was only a moderate reduction of the fibers in the field H1 of Forel.

Mesencephalon. The brachia of both colliculi had been cut on both sides and the laminations of the anterior colliculi were indistinct. The cerebral peduncles were entirely degenerated. The reticular substance was reduced somewhat in size.

Rhombencephalon. The only abnormalities were degeneration of the pyramids and of the fibers of the pons.

The first operation (on the left side September 8, 1931) resulted in a widely dilated and fixed left pupil. In other respects this more extensive unilateral ablation produced no functional change not seen after the first operations in 103 and 313. Nor did it alter the unusual exploratory activity, the friendliness and apparent enjoyment of petting that characterized this animal's normal state. The second operation (on the right side, December 23, 1931) was followed by a survival of almost thirteen months. For over a year this nearly hypothalamic cat survived in robust health. Considering the extensiveness of the cerebral removal it is not surprising that 228 proved to be the most abnormal of the four cats. She tended to move about less than the others and her walk was somewhat more abnormal; with head lowered, pelvis high, and hindlegs stepping from a wide base she moved in distinctly reptilian fashion. In the matter of taking food she was by far the most deficient. She never actually ate of her own accord and it was always necessary to feed her by spoon and pipette or, more conveniently, by rubbing her back while a dish of food was held below her mouth. She showed no trace of any visual or olfactory response. She failed to give the very specific responses to weak auditory stimuli evocable in the other cats and it is probable that their absence was due to the removal of the medial geniculate bodies. On the twelfth postoperative day and thereafter a most peculiar response to very loud noises such as a blast on a bugle or police whistle could be evoked. This consisted simply of twitching the skin of the back, erection of hair and widening of the palpebral spaces. Such were the specific peculiarities of this animal. In other respects she closely resembled the three cats already described.

The decorticate dogs.—The least extensive cerebral ablation was carried out in the case of dog 10. This animal was sacrificed four months after the second and final operation. Careful gross inspection of the brain showed that all neocortex had been removed with the exception of a small fragment situated ventromedially rostral to the septum. The greater part of the rhinencephalon was spared. The striatum was involved only laterally and although the thalamus had not been extensively damaged it had doubtless undergone the degeneration that it always shows following decortication. The hypothalamus and remaining portions of the brain stem appeared wholly intact. Dog 6 was sacrificed three months after the final cerebral operation. The brain presented essentially the same condition as that found in dog 10, but here no fragment of neocortex remained. Each pyriform lobe had been involved laterally in the removal.

In the case of dog 11 an attempt was made to produce a hypothalamic animal and, so far as could be determined by gross inspection of the fixed brain, the removal had been approximately the same as in cat 228. This dog was kept in good condition for about six weeks, but its continuous excessive activity, coupled finally with a gastrointestinal disorder (marked gastric dilatation, and a recently ruptured ulcer of the ileum were found at autopsy), led to extreme loss of weight, and death occurred during the eleventh week of survival.

Dogs 6 and 10 presented in general the condition made familiar by Goltz' description of 1892. Walking began on the second day but was never prolonged and showed certain abnormalities. The postural deficit was the same as that shown by the cats (4). Both animals responded to sounds in much the same way as did cats 103, 313 and 244. The only visual responses elicitable were pupilloconstriction and lid closure on illumination of the retina. These dogs shook themselves frequently in typical canine fashion. Scratching was often observed, but the movement was imperfectly directed.

In certain respects dog 11 differed strikingly from her two companions. Unlike them she never ate of her own accord

and swallowing seemed somewhat deficient. She was totally anosmic. Repeated attempts to evoke any response to changes in illumination or to a variety of sounds invariably gave negative results. The restlessness of this animal was extreme. Kept in a room about ten feet square she walked rapidly, in circles to the left, for hours at a time. If she encountered an obstacle she butted against it with effort and persistence until her head slipped to one side, whereupon she resumed her hurried and almost endless excursions. This extraordinary activity ceased only when the dog, apparently completely exhausted, lay down and at once went into a deep sleep from which she could be aroused only with difficulty. On waking the walking recommenced with renewed vigor.

Expressions of anger.—On the eleventh day following the final cerebral ablation and from that time forth (except when in poor condition) cat 103 displayed a stereotyped reaction when the tip of the tail was pinched. This consisted of: lowering the head, raising the back, drawing the ears back, loud growling, hissing, biting, vigorous alternate striking movements of the forelegs with claws unsheathed, turning (usually to the left), erection of hair, pupillodilatation, retraction of nictitating membranes, widening of the palpebral spaces, and cardiac acceleration (from 70–100 a minute at rest to 200–250 during activity). This reaction varied from the display of rage seen in normal cats, in being undirected. Although the striking was not at all incoördinate and the biting quite energetic, both were at first directed to the region just beneath the jaws. As the head was turned this attack was shifted to the animal's own flank, hip or hindleg. Pinching the tip of the tail could be done with impunity for neither claws nor teeth ever reached the point of stimulation. When the same reaction was induced by pinching a hindfoot or the skin of the flank the turning was usually toward the side stimulated, but even then the hand applying the stimulus was usually safe so poorly was the attack directed. It is significant that the animal never made any attempt to escape or move away while the stimulus was being applied. The

response never outlasted the stimulus; 'after discharge' was minimal. It was always safe immediately upon stopping the stimulus to place one's finger against the cat's mouth. Although a moment before, the animal had been viciously biting any object put within reach of its jaws the only result of this was to induce the licking which usually ensued when any object was placed in contact with the lips.

The ease with which the rage reaction could be induced in cat 103 varied from time to time. When there was an intestinal upset or when, as was the case toward the end of her career, large skin areas were infected the response as a whole could not be obtained; a strongly nociceptive stimulus induced only a growl. Over long periods during which the cat enjoyed excellent health there were variations in threshold and it proved impossible to correlate these with any definite circumstance. At times merely rolling the tip of the tail gently between thumb and forefinger sufficed to evoke a violent reaction. At other times a strong pinch was required. Occasionally the full reaction occurred when the cat was brushed or merely lifted, but usually such manipulations failed to induce any sign of anger. On several occasions 103 was tied on her back in precisely the same manner as were the cats of the earlier series of experiments (1). The reaction induced by this restraint was in every detail precisely the same as that described as sham rage. In order the better to appraise this behavior a large number of normal cats were subjected to the same restraint and the results convinced me that the responses shown by vigorous normal cats on being tied down differ in no detail from the sham rage of acutely or chronically decorticate cats.

Up to the tenth postoperative day rough handling or pinching failed to induce any sign of anger in cat 313. At the end of the second week and from that time to her sudden exitus four weeks later, light pinching of the tail or grasping of the loose skin of back or neck and, sometimes, mere gentle handling produced the same stereotyped rage reaction that was evokable in 103. The response was more violent and more easily provoked than in any of the other decorticate cats.

This fact may possibly be related to the extraordinary wildness and intractability that characterized this animal before the first and second cerebral operations. Yet the difference between the rage reactions of 103 and 313 in the decorticate state appear trivial when compared to the very great contrast in disposition exhibited by the two animals through long preoperative observation periods. My experience clearly indicates that decortication in cats and dogs is no respecter of individual traits.

The same rage reaction was evocable in cat 244 through the eleven months of survival, but it always required a very strong pinching of some part of the body to elicit it. When the animal was tied on its back it responded by a display of typical sham rage, but this was quite brief and after a few moments the animal remained quiet save for an occasional weak struggling fit. During the last two months of this animal's decorticate career she shared a room with a varying number of normal and partially decorticate cats. Among her companions were several quarrelsome beasts that made a practice of striking whenever she came within range. Their blows usually landed on her face or on the sides or top of her head. We were very much surprised to observe that 244 responded by holding her ground, raising the forepart of her body and striking out with one or both forefeet. During pauses between attacks (such pauses characterizing most forms of feline fighting) she kept one forefoot raised with claws protruded ready to strike again as soon as the combat was renewed. The striking was accompanied by retraction of ears, growling, spitting, erection of hair and the ocular signs of sympathetic discharge. Her blows were, of course, not directed and they were usually avoided by her more resourceful opponent, but when the latter pressed the attack by 'coming in close' she invariably struck harder and more frequently. It was soon discovered that gently tapping her on the nose evoked this same response and its quality made it advisable to wear gloves. It is noteworthy that she never ran away or retreated during one of these bouts. It is quite possible that the other surviving decorticate cats would have

shown the same behavior had they been exposed to the attentions of pugilistic companions, but for the sake of safety all three were kept by themselves. Since they had died or been sacrificed before this behavior was noticed in 244 the point was unfortunately not tested.

It will be recalled that cat 228 possessed rostral to the mesencephalon very little more cerebral tissue than the hypothalamic region which is essential for the sham rage of decorticate cats in the acute condition. Her capacity for displaying anger was therefore studied with considerable interest. On the sixth and seventh postoperative days pinching her tail caused loud growling, turning to the right, a definite pilomotor response, retraction of nictitating membranes and widening of palpebral spaces. There was no hissing, biting or striking. On the twelfth day the same stimulus produced spitting as well as growling, more pronounced turning, weak striking and maximal erection of hair of back and tail. Up to this time rough handling or brushing had failed to evoke any sign of anger. During the third week, however, brushing the back or flanks invariably produced the typical rage reaction. During the fourth and fifth weeks of survival this response was at its height—merely lifting her from the floor caused loud growling, spitting, clawing and the usual signs of sympathetic activity. The slightest pinching of the tip of the tail sufficed to give a violent reaction of the same type that was exhibited by 103. From the sixth to the tenth week a gradual diminution in the intensity and a rise in the threshold of the reaction were observed. By degrees brushing and handling became inadequate stimuli and the full reaction was induced only by a severe pinch or by the insertion of a thermometer into rectum or vagina. This relatively refractory state continued throughout the cat's long survival. The development and gradual subsidence of the capacity for exhibiting anger was not related to any changes in the general condition of the animal.

Each of the three dogs showed signs of anger when disturbed in certain ways. Gentle insertion of a rectal thermometer invariably caused growling, snarling (baring of

teeth) and struggling combined with snapping and biting. The same response was evoked in each by grasping the loose skin of the back. Occasionally merely lifting them gave this response. Gentle patting of a flank or hip invariably induced it in dogs 10 and 6, and occasionally in dog 11. The reaction was so easily induced in dog 10 that an attendant, used to ill-tempered animals, reported his unwillingness to continue handling the animal. Before the second cerebral ablation this animal had been regarded as the most gentle and friendly member of the dog colony. In the course of a study of their capacities for heat regulation (to be reported by Pinkston, Bard and Rioch) these dogs were frequently subjected to heat (38°–40° C.) and cold (1°–15° C.). It was very noticeable that signs of anger were more readily evoked when they were in the cold room and this was associated with increased restlessness. In the hot room the animals responded by remaining quiet (even dog 11 ceased her incessant walking) and a display of anger was then more difficult to induce.

A comparison of the rage responses of acute and chronic preparations.—In the earlier experiments (1) on freshly decorticate cats the sham rage was studied with the animal tied in the dorsal decubitus. This restraint was imposed because of the necessity for recording arterial pressure and heart rate. It doubtless played a rôle in the development of the sham rage. The capacity of those animals to show signs of fury under other conditions was not fully explored. It was found, however, that freeing them did not necessarily abolish the activity (see Part I, page 319). In the more recent work abundant evidence was obtained to show that in the chronically decorticate condition restraint on an animal board evokes a reaction which in every detail corresponds to the sham rage of the acute preparation. Furthermore, the same signs of intense fury developed in vigorous normal cats when they were subjected to the same restraint.

The excessiveness and easy elicitation of the sham rage suggest that the central mechanisms essential for its occurrence are in a state of overactivity or hyperexcitability. Thus it may be regarded as a 'release phenomenon' resulting from

the removal of an inhibitory control of cortical origin. But after any acute ablation of central nervous tissue the element of 'irritation' cannot be excluded. On the other hand any tendency of a healthy long-surviving preparation to react excessively must be attributed largely if not wholly to some sort of release. In the case of 103 there were fluctuations in the ease with which the typical behavior could be induced. The tendency to react to a given rage stimulus was sometimes greater, sometimes less than before decortication. During the fourth and fifth weeks of her survival cat 228 displayed anger under conditions which had proved ineffective as rage stimuli before the final cerebral ablation. This same oversensitivity was very prominent in 313, but it failed to appear in 244. The capacity to exhibit anger did not appear at once upon recovery from the anesthesia, but developed quite rapidly after a delay of from one to two weeks. In this respect the chronic differed strikingly from the acute preparations. It is probable that the long anesthesia, two to three days in duration, may account for the delay. The gradual regression of the rage reaction in 228 is of interest, but it does not seem possible to account for it on the basis of any known fact or set of facts. Although some secondary degenerative process may have been responsible, it is certain that degeneration of all severed nerve fibers had taken place before the change occurred. The animal was essentially a hypothalamic preparation and if one assumes that the overactive rage reaction was due to the release of a hypothalamic mechanism from some higher control one must make further assumptions to explain the regression in the reaction. Until much more work has been done it is useless to enter into a discussion of this question.

Expressions of fear.—On the eleventh postoperative day while cat 103 was being observed in a large open space it happened that the exhaust of an autoclave situated in an adjoining room was opened. The moment the loud noise of escaping steam was heard the animal suddenly retracted and lowered her head, crouched, mewed and then dashed off running rapidly in a slinking manner with head, chest, belly

and tail close to the floor. After blindly colliding with several objects in her path she came to rest in a corner where she crouched, mewing plaintively. During this activity and for some time afterwards the eyes were widely opened, the pupils were maximally dilated and there was some erection of hair on the back and tail. A few minutes after the noise ceased she resumed her previous behavior—walking slowly about licking the floor. A repetition of the same noise a little later produced the same striking reaction and it was also observed when a very similar noise was made by running water from a tap through a narrow nozzle.

This reaction was frequently obtained in 103 throughout her long survival. Loud noises proved to be the only stimuli capable of inducing it. Many sounds of low intensity (*e.g.*, a low whistle, scraping and clicking noises, the sound of an automobile horn in the distance) caused turning of the head and moving of the ears so that a listening attitude was assumed. It will be recalled that this auditory response diminished and soon disappeared when the stimulus was repeated a number of times, and the same proved true of the quite different response to loud noises. After much experimentation it was found that the most effective stimulus for this fear reaction was a loud blast on a bugle. The loud barking of an excited dog held just in front of the cat only produced crouching and retraction of the head.

In cat 313 a fear reaction of exactly the same nature was repeatedly obtained when a loud blast was blown on a bugle or police whistle. Less intense auditory stimuli proved ineffective. On the eighth day when signs of anger could not be obtained the fear reaction was evocable and it was repeatedly elicited during the animal's subsequent career. In 244 the reaction was confined to crouching, retraction of the head, plaintive vocalization, and widening of eyes and pupils. This animal never dashed off as did 103. No frank sign of fear could ever be induced in cat 228. When exposed to the noises that proved so effective in the case of 103 and 313 this cat merely twitched the skin of her back and exhibited erection of hair.

When normal cats were subjected to the blast of a bugle or the sound of escaping steam they reacted in very much the same way as did decorticate cats, 103, 313 and 244. A few merely crouched with head retracted, eyes wide and mewed as if frightened and bewildered. Some followed this maneuver by creeping away to hide in a far corner. Others at once dashed off precipitately and made wild attempts to escape from the room as if possessed by the most profound terror. Such observations lead to the conviction that it is entirely correct to describe the similar behavior of the decorticate animals as an exhibition of fear or terror. It is a very specific form of activity that cannot possibly be confused with any other mode of response. The element of escape which was completely lacking in the rage responses of these same animals was a most prominent feature of their fear reaction. Conversely, the element of attack which characterized their exhibitions of anger formed no part of the fear response.

Dogs 6 and 10 responded to the same loud noises that proved effective in the case of the three decorticate cats. The reaction consisted of retraction of the head, crouching low to the floor, crawling on belly and shivering. On the other hand dog 11, in which signs of anger could be readily evoked, never responded to any sort of auditory stimulus nor could signs of fear be elicited by other means.

These observations show conclusively that fear may be displayed in cats and dogs after removal of neocortex and much of the rhinencephalon, but they fail to delimit the subcortical region responsible for the display. Of some interest in this connection are the negative results obtained in cat 228 and dog 11. While cat 228 lacked all but a shred of the striatum and her thalamus proper had been almost entirely ablated, these structures were only partially injured in the cats that gave a positive result. Gross inspection of the brains revealed a similar difference between dog 11 on the one hand and dogs 6 and 10 on the other. At first sight these facts suggest that striatum or thalamus or both are essential for the production of the fear reaction. But when one con-

siders that the effective stimuli were auditory a more cogent explanation comes to mind. In cat 228, and apparently also in dog 11, the inferior quadrigeminal brachia, the medial geniculate bodies and the thalamic connections of the latter were removed whereas these important parts of the central auditory apparatus remained wholly intact in cats 103 and 313 and apparently also in dogs 6 and 10. In cat 244 where the fear reaction was somewhat less pronounced one medial geniculate had been extensively damaged. The nature and position of the central mechanisms concerned in the production of fear responses remain to be further elucidated and an effort is now being made to do this.

An instance of sexual activity after removal of the entire neocortex.—Many normal female cats living in the laboratory have come into 'heat' while under observation. The typical behavior is usually ushered in by the development of a marked playfulness which takes the form of rolling and rubbing the head. In elderly individuals this contrasts sharply with their usual mode of conduct. When fully in 'heat,' as indicated by congestion of the external genitalia and strong attraction for males, the female cat behaves in a very special manner. Resting on forearms and chest with pelvis elevated and tail raised the animal executes alternate treading movements of the hindlegs and emits a curious low sound not heard at other times. This posture and this action are maintained for hours at a time even when the cat is left entirely alone. It can be said of such animals that they are held bound by this pattern of behavior, for it is difficult, short of some excessive or unusual disturbance, to induce them to act in any other way. If now the vulval region be gently tapped or rubbed the treading is accentuated, the pelvis is further elevated and the tail is raised until it is perpendicular to the vertebral axis. If a male be present and does not at once approach, the female is likely to go to him and roll playfully before him. When the male is aroused he attempts to hold the female by the loose skin of the back of the neck and this usually induces a certain amount of spitting and growling.

This brief account of the sexual behavior of the normal

female cat will, it is hoped, make clear the significance of the following observations. On the twenty-ninth day following the final cerebral removal in cat 103 the insertion of a thermometer into the vagina immediately induced loud growling, lowering of head and chest, elevation of pelvis and tail and treading movements of the hindlegs. This behavior, identical, except for the growling, with that shown spontaneously by normal cats in œstrous, was maintained for the few moments during which the thermometer remained in contact with the genitalia. On removing the instrument the cat rolled over onto its side and with face upwards playfully rubbed the back of her head and neck against the floor. This sequence of events was repeated on again inserting the thermometer. Furthermore, the typical posture of œstrous was assumed and the treading occurred whenever the vulval region was tapped or rubbed. No other form of stimulation produced this behavior. It is significant that insertion of a thermometer into the rectum never evoked it.

The capacity of responding to direct genital stimulation by activity identical with that which occurs spontaneously in normal females during œstrous lasted in cat 103 from the twenty-ninth to the fifty-eighth postoperative day. It recurred for a few days during the sixth month. At all other times during the twenty-eight months of survival it could not be induced. During the first sexual period vaginal smears were taken and these showed a cellular picture typical of œstrous. Later when sexual behavior could not be induced smears showed no such indication of œstrous.

Cat 103 attracted males only when the typical posture with treading could be evoked by artificial stimulation of the genitalia. At these times her proximity provoked the most intense sexual excitement in almost every male that was placed in the same room with her. Four of the active males were still in the laboratory when 103 no longer showed either the typical reaction to genital stimulation or the œstrous type of vaginal smear. At this time, although responsive to normal females in heat, they repeatedly failed to become excited by her presence.

When she became the object of the attentions of a sexually excited male the behavior of 103 was most interesting. On being put with her the male usually indulged in some preliminary advances such as rubbing his head against her neck or head, licking her face, shoulder or neck. To these stimuli she appeared entirely indifferent. But if in exploring with his nose he happened to touch the labial region she at once responded by lowering the forepart of her body, elevating pelvis and tail and treading. When the male mounted and executed copulatory movements she showed no sexual response unless, as occasionally happened, this procedure was accompanied by mechanical stimulation of the labia. In the absence of such stimulation she merely crouched, hissing and growling as he pulled her about by the loose skin of the neck. Under these circumstances the normal female in œstrous assumes the typical posture even before the male attempts copulation. The lack of response of 103 to any activity of the male except direct genital contact made actual copulation difficult. However, on three occasions it was thought that intromission had actually occurred and this impression was confirmed by subsequently finding spermatozoa in vaginal smears. On each of these occasions, but at no other time, 103 turned over onto her side, rolled and playfully rubbed her head with forepaws.

From these observations it is clear that a behavior pattern typical of the female cat in œstrous was evoked after complete removal of the neocortex. However, the reaction which appears spontaneously during œstrous in the normal female, was elicited in this decorticate cat only by direct sexual contact. Whether spontaneous sexual activity can develop after decortication remains to be determined. Some recent experiences indicate that decorticate cats and dogs may be thrown into 'heat' by administration of œstrogenic substances. Cat 103 was the only one of the seven decorticate animals (all females) that showed any signs of sexual activity. The double ovariectomy performed early in the preoperative observation period was not complete, for at autopsy a small mass of luteal tissue was found. Doubtless at the time of

œstrous, two years earlier, some ovarian tissue containing mature follicles was present.

The absence of signs of pleasure.—On many occasions persistent attempts were made in the case of each of the four cats to evoke signs of pleasure. Stroking them or rubbing their heads never induced such signs. Sometimes rather hard rubbing of the head at the side just below the ear induced inclination of the head to that side, but this was almost always followed by attempts to scratch the stimulated area with the ipselateral hindfoot and it evidently was nothing more than a part of the scratch reflex. Purring was heard only once. Cat 244 while lying on a comfortable cushion was heard to purr for a few minutes while thermocouples were applied to the skin of her flank for the purpose of measuring surface temperature. This procedure was carried out under the same circumstances hourly throughout several twenty-four hour periods, but only once did purring occur. None of the other cats ever purred when made comfortable and gently petted. As a matter of fact purring in cats occurs under many different conditions and it is not uncommon to hear it in cats when they are agitated or restless; in fact I have frequently heard loud purring in cats that were definitely angry. Thus it often accompanies tail-lashing, growling, snarling and may be heard in the intervals between rapidly repeated attempts to bite and scratch. In the case of the dogs petting never induced tail wagging or other signs of enjoyment; in fact unless gentle it often induced anger.

III. Emotional Expression after Partial Decortication

In the series of more than thirty unilaterally decorticate cats expressions of anger, fear and sexual excitement remained unaltered. The same can be said of an equally large group in which partial removals of one cortex had been carried out (see 4). Bilateral removal of the frontal poles (premotor cortex and motor cortex) also failed to produce any perceptible change in the threshold or quality of reactions of anger or fear. These results are not in harmony

with the view that ablation of the frontal regions is followed by an increased tendency toward emotional display. Fulton and Ingraham in a preliminary communication (16) have reported that in cats the surgical interruption of a postulated pathway from frontal lobes to hypothalamus results in a condition in which rage is much more easily elicited than in a normal animal. The provocative lesion was a transverse cut 2–3 mm. deep made at the base of the brain 3–4 mm. anterior to the chiasm. It is not unlikely that these interesting results will be explained on some other basis than removal of a frontal influence.

When ablation of the cortex and varying parts of the brain stem on one side was combined with extirpation of only the frontal pole on the other a most definite change of character ensued. Eight such animals have been prepared and each has been studied for months. Two of these, cats 168 and 237, have already been described (4). Regardless of previous temperament this bilaterally unequal ablation has always profoundly changed the character of the animal. Each has shown excessive curiosity and what may be regarded as an inane desire for human companionship. These cats invariably ran to anyone entering their quarters and then followed tenaciously. With surprising regularity they rushed toward any object moving in their remaining visual field or toward the source of certain sounds. But in no case have these animals become bad tempered as a result of the operations. Their most obvious deficiencies have been postural and they have invariably failed to care for their coats. When the only cortex remaining has been one frontal pole the animals have been far more normal. Their postural abnormalities were confined to the legs opposite the completely decorticate hemisphere and, in great contrast to wholly decorticate cats, they kept themselves neat and clean. They showed a somewhat increased tendency to become angry on being handled and they picked fights with other cats. But it cannot be said that these subtotal decortications rendered the animals excessively prone to exhibit rage.

Summary of Parts I and II

1. The term sham rage describes the *expression* of anger occurring after decortication. This motor activity is dependent on the caudal hypothalamus. Although signs of anger may be evoked after mesencephalic transections of the neuraxis, a hypothalamic mechanism contributes a prepotent factor for the expression of this emotion.

2. Sham rage develops under conditions that elicit definite rage responses in normal cats. Like them it is a specific mode of response, a behavior pattern, and like them it is readily distinguishable from other forms of emotional expression, *e.g.*, fear reactions and sexual behavior. It has been incorrectly described as a state of excitement (Harlow and Stagner).

3. Two distinct forms of emotional behavior, exhibitions of fear and anger, have been repeatedly evoked in long-surviving decorticate cats and dogs. An instance of sexual behavior occurring in a female cat without neocortex is described.

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[MS. received February 27, 1934]

REWARD AND PUNISHMENT

BY E. R. GUTHRIE

University of Washington

One of the most ancient and one of the best established of beliefs concerning learning is that learning is determined by its effects. Common sense states this rule in terms of pleasure and pain. Action leading to pleasure tends to be fixed as habit, while action leading to pain tends to be 'stamped out.'

There can be no real quarrel with this popular theory. Spanking and caressing have been justified in these terms for an undoubtedly long time. The theory is well established.

But the popular theory has several defects for scientific purposes. In the first place when it is applied to animals we become a little puzzled as to how to tell pleasure and pain. And unless we can define them so that they can be unambiguously recognized they are of no use to a theory of learning; for a theory of learning must state the observable circumstances under which learning occurs and under which it fails to occur. Habits are formed by snails and earthworms, but no one has even tried to describe what a pleased snail or a pained earthworm looks like. Pleasure and pain in dogs would seem a little more readily described, but we find that even in human beings pleasure and pain are not easily recognized, and there are instances of learning to respond to stimuli which human subjects did not know were present and so would obviously be themselves unable to report the experience of these unperceived stimuli as painful or pleasurable.

We need then to substitute for pleasure and pain as the conditions of learning some less ambiguous condition, some condition that can be easily recognized by an observer, because we can not always ask the learner which he feels, and can not depend very much on his answer when he gives it.

This less vague and less ambiguous description of the

conditions of learning has been suggested by Thorndike. He would substitute for pleasure and pain, satisfaction and annoyance. These are the conditions that determine the fixing and unfixing of habits. He defines satisfaction and annoyance in strictly behavioristic terms so that they can be readily recognized by any observer. "By a satisfying state of affairs," Thorndike[1] says, "is meant roughly one which the animal does nothing to avoid, often doing such things as attain and preserve it. By an annoying state of affairs is meant roughly one which the animal avoids or changes." His principle, which he calls the law of effect, is as follows:[2] "When a modifiable connection between a situation and a response is made and is accompanied or followed by a satisfying state of affairs, that connection's strength is increased: When made and accompanied or followed by an annoying state of affairs, its strength is decreased. The strengthening effect of satisfyingness (or the weakening effect of annoyingness) upon a bond varies with the closeness of the connection between it and the bond. This closeness or intimacy of association of the satisfying (or annoying) state of affairs with the bond in question may be the result of nearness in time or of attentiveness to the situation, response and satisfying event in question."

This statement of the law Thorndike has recently somewhat amended, in line with an admirable tendency to be guided by the facts rather than by his theory, a tendency which he has often demonstrated. He no longer believes that the action of annoyers is the opposite of the action of satisfiers in all respects. The strengthening effect of satisfaction is more universal, more inevitable, and more direct than the weakening effect of annoyance. An animal that gets food by pulling a loop learns to pull the loop. "But if an animal," he says,[3] "in the same situation pulls a loop and either (*a*) gets a shock in its paw at contact with the loop, or (*b*) gets a blow on the back, or (*c*) gets a sudden pain in the bowels,

[1] E. L. Thorndike, The fundamentals of learning, New York, 1932, p. 176.
[2] *Ibid.*, p. 176.
[3] *Ibid.*, p. 276.

the weakening of the connections is likely to vary. In (*a*) there will probably be much weakening by way of strengthening the connection between the situation and the response of drawing back from the loop. In (*b*) there will probably be weakening, but less, because the reaction will probably be jumping away from the place, which is not so inconsistent with pulling at the loop. If the animal in (*c*) reacts by screaming without letting go of the loop, there may be no weakening at all."

In other words Thorndike's view now is that punishment may or may not lead to unlearning depending on what it causes the animal to do. The important thing to be noticed is that Thorndike's amended explanation is precisely in terms of the conditioning which he rejects as an explanation. The sight of the loop will later cause the animal that hurriedly withdrew its paw on being shocked to withdraw its paw on the next occasion. The animal that gets a blow on the back will on the next occasion tend to jump away. The animal with the gripe in its bowels will tend to do whatever it did in response to the gripe. That this is an appeal to pure conditioning is concealed by putting the emphasis on the loop pulling and speaking of this as weakened or strengthened. We can be, as Thorndike acknowledges, much more precise than to predict the failure of loop-pulling. We can predict the recurrence of the specific behavior indulged in.

How it escaped Thorndike's notice that the effects of satisfaction are likewise readily and much more precisely described in terms of conditioning is hard to understand. Satisfaction is defined as a state of affairs the animal does nothing to avoid or often acts to maintain. This is to say that a situation in which a maintained response is made on later occasions tends to evoke that response. 'Doing nothing to avoid' a state of affairs does not mean doing nothing whatever; it means maintaining orientation, maintaining attention, and on the second occasion the general facts of conditioning would lead us to expect a repetition of the behavior.

In cases of hunger where the satisfaction is a satisfaction of some source of unrest, maintained hunger spasms or a continu-

ous painful stimulation, it is quite reasonable to suppose that the precurrent behavior is associated with the hunger, but that this association is continuously destroyed by new associations. There is one act, however, to which hunger may remain a faithful conditioner. That is the act of eating. And the faithfulness of hunger to this association derives from the fact that hunger dies when eating occurs. As Stevenson Smith and I pointed out in our General Psychology, elements of the consummatory response tend to be present throughout a series of actions driven by a maintaining stimulus. Hull's [4] paper on his goal-gradient hypothesis describes the rôle that such traces of the consummatory reaction may play in guiding learning and in holding the animal to its 'purpose.' Washburn [5] had called attention to the 'gradient' shown in the learning of a maze from the movements just before final success to those at the beginning of the series. In her account the drive serves as the possible cue to which the movements just before consummation are soonest attached. Hull points out many other possible cues.

Thorndike explains the appearance of retroactive effect—the reinforcement by a satisfying outcome of an association that lies in the past, by suggesting a physiological event in the brain. Of this he says that "the evidence that it is some condition favoring conduction across certain synapses is still strong." [6] "The physiological equivalent of a connection does not thus vanish utterly in the twinkling of an eye. Whatever it is, it is there a second after it occurred in a manner or degree quite different from that of a connection of an hour ago." [7] This cerebral hangover is a highly speculative and quite unnecessary assumption. The physiological attendant of a connection which 'does not vanish utterly in the twinkling of an eye' may well be the maintained muscular contractions of the response. We know that these tensions in systems of muscles may be maintained for long periods. And while

[4] C. L. Hull, The goal gradient hypothesis and maze learning, PSYCHOL. REV., 1932, 39, 25–43.

[5] M. F. Washburn, The animal mind, 3d ed., New York, 1926, p. 335.

[6] E. L. Thorndike, Fundamentals of learning, New York, 1932, p. 314.

[7] *Ibid.*, p. 314.

they are still maintained they are reasonably certain to be subject to conditioning. The action that produces 'satisfaction' is not over 'in the twinkling of an eye' and by Thorndike's own definition of satisfaction it is an act that is maintained, or at least not broken up by the vigorous interference of punishment. Peterson [8] in 1922 wrote ". . . Nerve impulses flash through the organism in but a fraction of a second. But there is considerable evidence to show that the effects do not so immediately fade away. Probably the responses of muscles and glands set up other afferent nerve impulses, which . . . bring about further responses." And "These streams of impulses therefore will exist contemporaneously with subsequent stimuli and exert important directive influences on the nerve impulses these stimuli set up."

"The influence upon learning," Thorndike [9] says "of both satisfiers and annoyers depends upon what they cause the animal to be or do." This is exactly what the writer is suggesting, namely, that the future response to a situation can be best predicted in terms of what an animal has done in that situation in the past. Stimuli acting during a response tend on later occasions to evoke that response.

I would not hold that all satisfiers tend to fix the associative connection that has just preceded them. When a satisfying situation involves breaking up the action in progress it will destroy connections as readily as punishment. In teaching a dog to sit up, tossing his rewarding morsel to a distant part of the room will prove a very ineffective method. There is no doubt of the satisfying character of the meat. The dog certainly 'does nothing to avoid, often doing such things as attain and preserve,' not, of course, the meat, but the eating of it. But the effect of the reward will be that the dog instead of sitting up stands ready for another dash across the room.

Just as satisfiers do not always 'stamp in' a connection, so annoyers do not, as Thorndike himself has perceived, always 'stamp out.' What we can predict is that the influence of the stimuli acting at the time of either satisfaction or annoyance

[8] J. Peterson, *J. Exper. Psychol.*, 1922, 5, 270.

[9] E. L. Thorndike, Fundamentals of learning, p. 312.

will be to reestablish whatever behavior was in evidence at the time.

"A satisfier," Thorndike[10] says "which is attached to a modifiable connection always, or almost always, causes the animal to be or to do something which strengthens the connection to which the satisfier is attached; but we do not know what this something is. It may be to maintain relatively undisturbed the physiological basis of the connection; it may be to retain it longer than would be otherwise the case; it may be to confine it by some metabolic effect; it may be to alter it in some more mysterious way." I suggest that the mystery may be reduced by supposing conditioning to have taken place. This is, of course to invoke another mystery but one somewhat less mysterious because more familiar. The something that the satisfier causes the animal to do on the second occasion is the repetition of its behavior on the first occasion—always allowing for possible new elements that may interfere. The dog's lesson in sitting up may be always interrupted by the cat. If it is objected that this explanation seems to demand a retroactive effect on connections we can only say that 'backward' association in the sense that the cue may follow the original stimulus is well established. This retroactive effect need not be actually anything more than simultaneous association as Washburn [11] has suggested. The substitute stimulus is probably always coincident with the response, though not with the original stimulus.

To this last quotation Thorndike adds a footnote: "The satisfying after-effect obviously often causes the animal then and there to continue or to repeat the connection." So long as the substitute cue for action remains, this would seem a very natural consequence. One taste leads to another because the stimuli are still present unless the animal turns away from the food, which would be by definition the work of an annoyer. As in the case of the chick and the cinnabar caterpillar, rejection, the result of the bitter taste, may be conditioned on the sight of the caterpillar, and replace the original impulse to

[10] *Ibid.*, p. 312.

[11] M. F. Washburn, The animal mind, New York, 1926, 3d. ed., p. 232.

peck. This is not a 'retroactive effect' though it has that appearance. It is simultaneous conditioning. I venture to predict that learning would be much more uncertain if the caterpillar were of such a size that it was swallowed at one peck and not visible while rejection was going on.

We may go on to inquire how any stimulus becomes an annoyer in the first place, a question which Thorndike does not consider. He defines an annoyer "as a state of affairs which the animal avoids or changes." But this ability to avoid is just what it is necessary to explain. Hammering the thumb or bumping one's head on a beam are not annoyers according to this definition unless we assume that the learning has already occurred, for the victim can not avoid them after they have happened. If he avoids them at all it must be in time, and this implies that the learning which we hoped to explain has already taken place. We may, for the sake of argument, consent not to be annoyed by such events and deal only with annoyances which satisfy the definition, that is to say, with continuous stimuli which we can do something about while they are still upon us, such as intense heat, a bumpy road, thirst, hunger, flies, radio programs, or, if we are laboratory animals, charged grids, immersion in water, confinement.

Now we do not know, unless we observed it on some previous occasion, what either animal or man will do in any of the above situations. Holt [12] has well argued that the early and primitive response to such stimuli is approach. If the fingers are flexed the finger tips touch the palm. The touch on the palm then comes to be a conditioner of the finger movement. Holt suggests that the grasping reflex is thus learned before birth. In general the stimuli caused by movements come to be the conditioners of the movements which cause them. Holt has also described how intense stimuli may, through conditioning, become stimuli for withdrawal and avoidance. Annoyers are essentially intense stimuli. Their original effect, before we have learned to avoid them, is in some cases approach, but in all cases excitement. Intense stimulation brings about general tonus of skeletal muscles and

[12] E. B. Holt, Animal drive and the learning process, New York, 1931.

reinforces action. We do not know what a man or an animal suffering an intense stimulus will do, but we do know that he will not be relaxed and that there will be variety and energy in his actions. He will be active, and being active his activity will be varied, because activity changes the stimuli which act upon him. Eyes and ears, as well as muscles and tendons will be subjected to rapid change in stimulation. If these intense stimuli responsible for his excitement are maintained (we have elsewhere referred to them as maintaining stimuli) as they are in those situations which would really annoy Thorndike, they have opportunity to become the conditioners of many and varied actions, but each successive action alienates them from its predecessor.

There is one act, however, to which these maintaining stimuli may remain faithful conditioners. This is the act which removes them. The maintaining stimuli are no longer present with the succeeding acts and so may remain conditioners of the movement which took them from the victim, or took the victim from them. If annoyance means avoidance, we had to learn to be annoyed at annoyers. At first they were only disturbers.

Dunlap [13] treats reward and punishment in part in terms of feeling, rather than of satisfaction and annoyance. "That feeling in itself," he says, "without thought, is of any importance is improbable, except in so far as feeling may be an organic condition which is generally favorable or unfavorable to learning. From present information, we may infer that perhaps mild feeling is favorable and intense feeling possibly unfavorable, but we have no indication that any specific type of feeling is any different in its effect from any other, except the feeling which is involved in desire."

To this it may be suggested that the effect of intense feeling is not unfavorable to learning in general but very favorable. But what is learned will be what is done—and what is done in intense feeling is usually something different from what was being done. Sitting on tacks does not discourage learning. It encourages one in learning to do something else than sit. It

[13] K. Dunlop, Habits, their making and unmaking, New York, 1932, p. 30.

is not the feeling caused by punishment, but the specific action caused by punishment that determines what will be learned. In training a dog to jump through a hoop the effectiveness of punishment depends on where it is applied, front or rear. It is what the punishment makes the dog *do* that counts or what it makes a man do, not what it makes him feel. The mistaken notion that it is the feeling that determines learning derives from the fact that often we do not care what is done as a result of punishment, just so that what is done breaks up or inhibits the unwanted habit.

My own view of the way in which unpleasant or unsatisfactory consequences alter subsequent behavior may be further illustrated by a minor incident in the routine of a certain prominent psychologist. He rented an apartment for the summer with a garage which had a large, swinging door. From the top of the door hung a heavy chain. When the psychologist opened the door hurriedly the first morning, the chain swung about slowly and struck him a blow on the side of his head, a distinctly painful and 'unsatisfactory' event. But this continued to happen each morning for some two weeks. Why the long delay in learning to stand aside?

The answer, I believe, is that the act of opening the door was performed while looking at the exterior of the door. The chain struck after the door had opened and the scene changed. Dodging was not conditioned on the sight of the door because a sight of the door had not accompanied flinching from the blow. The flinching movement which occurred as the rear of the car came into view was too late. Only after the incident had been talked about and finally had been told to a visitor on the way to the garage did caution show itself in time.

The whole incident is not to be explained in terms of pain, or annoyance, but in terms of the action and its cue. It is not the annoyance, but what the annoyed person does that determines what will be learned. Annoyance, in so far as it means increased muscle tonus and more complete and vigorous action, is favorable to learning to do whatever is done in response to whatever cues are present. The mistaken belief that annoyance discourages learning comes from placing all the at-

tention on one line of action. Annoyance often accompanies the sudden disruption of an activity and leads to 'unlearning' that activity, but at the expense of learning something else.

For an excellent account of the history of pleasure-pain theories of learning the reader is referred to an article by Cason[14] in the PSYCHOLOGICAL REVIEW. The theory had been elaborated by Herbert Spencer in his Principles of Psychology. Cason's opinion that a logical error is involved in the statement that what comes after an activity has a retroactive influence on the activity, I can agree with only in a very narrow sense. Because actions are spread out in time and because stimuli also may act for a period rather than for an instant, the actions that accompany pain or annoyance may be anticipated on a second occasion and so break up a habit series. Only in this sense is the pleasure-pain theory of learning correct. It is a very rough and inadequate description of the facts, which are more adequately predicted in terms of the conditioning of specific behavior.

There is nothing in this discussion which should deprive punishment and reward of the place they hold in public favor. No doubt whatever has been thrown on their effectiveness. Children may still be spanked or caressed. But we shall have a much better insight into the uses of punishment and reward if we analyze their effects in terms of association and realize that punishment is effective only when it reconditions new responses to the cues for unwanted behavior and reward is effective only through its associations. Punishment achieves its effects not by 'taking away strength from the physiological basis of the connection,'[15] but by forcing the animal or the child to do something different, and thus establishing inhibitory conditioning of unwanted habits. Punishment is effective *only in the presence of cues for the bad habit.* The law of effect would not have made us aware of this. Furthermore, when the effect of punishment is only emotional excitement, punishment facilitates the stereotyping of the undesired habit.

[14] H. Cason, PSYCHOL. REV., 1932, **39**, 440–466.

[15] E. L. Thorndike, The fundamentals of learning, p. 313.

Punishment and reward are essentially moral terms and not psychological terms. They are defined not in terms of their effects on the recipient, but in terms of the purposes of the individual who administers them. Theory stated in their terms is bound to be ambiguous.

[MS. received May 9, 1934]

SCIENTIFIC METHOD AND THE EXISTENCE OF CONSCIOUSNESS

BY DONALD C. WILLIAMS

University of California at Los Angeles

It is the strangest anomaly of recent science that while an influential number of physicists, once supposed to be students of physical nature, are suggesting that only conscious experience exists, an equally influential number of psychologists, once supposed to be students of consciousness, have suggested that only physical nature exists. Either of these two contrary propositions seems paradoxical to the non-professional; but perhaps the greatest impatience was provoked by the psychologist's, both on the side of the behaviorists who defended it and on the side of the non-behaviorists who repudiated it. Today, however, the contention has lost newness and acerbity, and it may be time to attempt an analysis to determine precisely the import of the controversy.

At best, inquiries about *existence* seldom occur in science, but the polemic over consciousness has been confused by an exquisite and almost studied ambiguity (**50**). A fair composite of typical statements by Watson (**40**, 27–28), Weiss (**45**, 304; **47**, 273), Markey (**26**, 385), Hunter (**17**, 16), Tolman (**36**, 360; **39**, 215, 417), and Boring (**6**, 118), for instance, would be like this: "For the behaviorist, consciousness as a unique non-physical substance does not exist." To ask of the truth of such a declaration incurs the logical fallacy of many questions. Does it mean that consciousness does not exist, or that the behaviorist ignores its existence? That he denies (or ignores) that it exists or only that it is a *substance?* Or only that it is *unique?* Or only that it is *non-physically* unique? And what, after all, is meant by 'consciousness' in such a context? A statement about consciousness is logically impossible, I suppose, only if it contradicts itself, but it is genuinely 'absurd' and 'scientifically impossible' if, without

conveying any alternative advantages, it entails the contradiction of what all reflective and scientifically trained men, including at his other moments the author himself, have considered either indubitable or at least more certain than anything else. Of the several interpretations of the ambiguous thesis of behaviorism, some are paradoxical and impossible in the foregoing sense. Some are, in the present state of science and philosophy, the merest platitudes. Some are significant and instructive.

Any disquisition on 'consciousness' must first indicate what world-element, real or fancied, the word is to denote. Now, in the first place, the sense in which we customarily ascribe consciousness to that which is aware of something, to a 'conscious animal,' for instance, is apparently quite different from the sense in which we ascribe consciousness to that of which something is aware, to a 'conscious state' or a 'conscious emotion,' for instance (**35**, 171). The distinction has been hypostatized by, although it does not require, psychological and epistemological theories of 'act' and 'content.' In English, it is represented by the difference between 'sentient' and 'sensuous' or between 'percipient' and 'percept.' Both of these pairs of terms, however, have too special a connotation, and on a further lame analogy to the Latin participles I propose to speak of '*conscient* animals' or 'acts' and of *consciency* as their defining attribute, while I continue to speak of '*conscious* states' and of *consciousness* as their defining attribute.

Taken strictly, 'consciousness' should therefore mean the abstract and universal character which inheres in all things which are conscious. Actually, however, the English language has a genius for perverting abstract and universal terms to relatively concrete and particular uses, and 'consciousness' in even academic circles is perhaps most frequently used to mean a collection or class of conscious entities. 'Consciousness,' then, as I first treat of it below, means the total content of conscious experience, my own or other persons' as the context requires.

A third and equally annoying point of usage consists in

the fact that the names of all modes of consciency, general or specific—as 'aware of,' 'knowing,' 'experiencing,' 'perceiving,' 'observing,' 'seeing,' 'smelling,' etc.—are used to indicate sometimes the conscient being's relation to what is immediately presented, 'the conscious content,' and sometimes its relation to what is mediately cognized, 'the real object.' The distinction between these two is relevant on any conceivable theory of knowledge, even on the phenomenalistic or neo-realistic, for which the content is supposed to be a part, but only a part, of the object, and it is indispensable to any clear discussion of scientific method. The word 'consciousness' shall indicate hereafter the immediate content of experience, and 'consciency' the immediate awareness of it. All transitive verbs representing particular modes of consciency, however, such as 'observe,' 'see,' etc., shall, if unmodified, mean mediately observe, mediately see, etc. (*i.e.*, shall indicate the relation to the concrete object), and only where these verbs are modified by an adverbial prefix, as 'immediately observe,' 'immediately see,' etc., shall they indicate the relation to the content itself.

I. Consciousness in the concrete is notoriously difficult, but not impossible, to define. A complete psychology might define it analytically by its structure. Discussions like the present help to define it synthetically, by its place and function. But to begin with, it is simplest to define it denotatively by reference to examples and varieties of it. I shall use the word, like Sellars (**32**, 406–407), "in the traditional sense as a denotative term for the total field of a person's experiencing as it shifts and changes": *i.e.*, for all which is denoted as the conscious content, the stream of consciousness, states of consciousness; immediate experience, direct experience; 'thoughts,' 'feelings,' or 'ideas' in the colloquial use; the intuited, the data, the given, 'raw feels,' the *sensa*, the sensibles, presentations and representations; the specious present; the mélange of sensations, images, and affections of structuralistic introspection; the evidential data of the scientific positivist; the phenomenological field of the *Gestalt* psychologists; the 'apparitions' of Hobbes, the 'phenomena' of Kant, the 'ap-

pearances' of the idealist, the 'elements' of Mach, the 'phanera' of C. S. Peirce; the immediate colors and smells and toothaches, thrills and pleasures, tickles and distresses and desires, which make the tapestry of experience. Men give it this name and that, and may interpret it as 'things' or as 'thoughts,' but with the *stuff* which these words denote most men seem to profess complete familiarity. The easiest, while the most paradoxical, interpretation of the behaviorist's thesis is that he means literally to deny the existence of everything indicated by the foregoing phrases—to declare that they are in fact as meaningless as the wrinkles on a withered apple. The non-behaviorist usually dissents so profoundly and instantly from this supposed doctrine that he can hardly focus on it or express himself save by angry expostulation or amused indulgence (**22**, 3). But if it is indeed 'absurd' and 'scientifically impossible,' in the way previously defined, it should be capable of being reasonably shown so.

A. Now, in spite of all emphasis upon the public and coöperative character of science, it is the individual intelligence which must in the last analysis pronounce judgment upon any specific doctrine. If I consider first, therefore, what I have learned to call 'myself,' doubt of the reality of conscious experience seems impossible. On my most immediate observation and most honest information and reflection, what I call 'myself' is, principally, what I call 'conscious experience.' Within this field or stream of experience all my knowledge grows. The field provides, first, all the occasions which suggest or require the hypotheses of common sense or of science—the data to be explained, the problems to be solved. It contains, second, all the conceptual systems and hypotheses which attempt to solve these problems. It contains, third, the judgmental processes which verify or reject hypotheses. It contains, fourth, all the evidence by which my hypotheses are verified or refuted. And first, last, and always, it contains all the patterns and qualities by which I *define the terms* of my discourse. 'Red' and 'twice' and 'response' may be applicable beyond immediate experience, but they got their meaning, in the first instance, by denoting elements and aspects in

experience. Not only does the conscious content exist; it is for me the very type of existence, of the *fact;* and the relation between the proposition "conscious experience exists" and this fact is for me the type and ideal of *truth.* All other material propositions have, on any evidence, a fractional probability-value between 0 and 1. Only the proposition that the ultimate evidence exists has, on the evidence, a value of 1, and only the proposition that the evidence does not exist has a value of 0. My consciousness, in sum, far from being 'assumed' and 'unobservable,' as some behaviorists complain (**41**, 5; **42**, 1; **43**, 3; **44**, 628), is the only thing wholly 'empirical' and immediately 'observable' (**11**, 268; **20**, 27), and far from my wanting things 'explained with consciousness left out,' as some behaviorists offer, my conscious experience is what primarily I want explained.

I have spoken of 'me' always in the faith that the reader can with complete acquiescence read his own 'me' in place of mine. Nevertheless, it is fair to ask how I know that other persons exist, that they have conscious experience, and that it is, like mine, the source and stuff, if not the sole object, of all their knowledge. This question, raised by the 'unphilosophical' behaviorist, is the old philosophical problem of how we know other minds. The gist of the answer is that although I cannot be as sure of other persons' experience as of my own, I can reasonably be as sure of it as of anything else.

Notoriously it is theoretically possible to doubt *everything* beyond one's own mind, and the behaviorist, grotesquely enough, seems sometimes to indulge this possibility (**31**, **33**, **46**, **47**). We shall assume, however, that material bodies exist, because if they do not there is no behavior and no behaviorism. With this stipulation, the behaviorist may ask me to believe that beyond the ring of *my* conscious experience there exist bodily organisms similar to that which conditions my conscious experience but unaccompanied by any conscious experience of their own: engaged, none the less, in docile behavior at the instance of circumstances, stimulating one another with swords and pens, being born and mating, building universities and penitentiaries. This suggestion, like James's

(19) and Bode's (3) myth of the automatic sweetheart, or Bayliss's (1) of the automatic Einstein, or Descartes's of automatic dogs and cats, is a conceivable hypothesis. It is self-consistent, unless on some such theory as Lashley's, that consciousness literally *is* a bodily event (24). It does not directly contradict *my* evidence, and I certainly have no scientific confidence in that 'intuition' or *Einfühlung* some gregarious scholars have supposed directly to assure us of the existence of other minds. The hypothesis is *possible*, but its scientific value and its probability are as near *nil* as those of any hypothesis could be.

The ordinary processes of inductive inference, the abstraction, analogy, generalization, and extrapolation which are the only processes which empirical science has, inform me that other men are conscient as they inform me that other men have bodies or that I have a body or that I shall have a headache (13). Nor am I left to make a lonely inference solely from my own experience. All the inductive criteria approve, and all science, including particularly the science of behavioristic psychology, must accept, the assumption that other men can and do make veridical verbal reports to me, and that the language which I ascribe to them has the same significance as the language I ascribe to myself. And all these human organisms, not simply those of priests and poets, but those of the scientists whom the behaviorist most approves, agree in telling me not only that they are in enjoyment of conscious experience, but that it plays in their lives the unique part it does in mine. To suppose that other men do not have conscious experience would be to brand as pointless delusion or deception both the idlest comment and the most solemn assurances of my friends and my family; of nearly every page of all the literatures of the earth; of the greatest philosophers and especially the most modern philosophers; of the religious teachers and of the moralists. It would brand as pure idiocy the whole literature of introspective and experimental psychology, and the conviction of the great psychologists, past and present, that, as Köhler puts it (20, 25), "direct experience . . . is the only basis upon which I can continue to guess about

physical realities." It would contradict all of modern inductive logic, the logic of science, for which, as Boring writes (**5**, 177), "the initial position of experience in the scientific process can not be denied. It is the starting point of all empirical, and therefore experimental, inference." It would contradict the assurances of modern physicists about the method and nature of the science which behaviorists praise as the *beau idéal* of knowledge, of which Planck says (**29**, 66) that "the beginning of every act of knowing, and therefore the starting-point of every science, must be in our own personal experiences . . . the first and most real hook on which we fasten the thought-chain of science," and of which the more radical Poincaré, Campbell, Jeans, Eddington, Bridgman, and Lenzen have predicted a denial of the existence of everything except conscious experience. It would contradict the doctrine of the less extreme behaviorists, on other matters given respectful credence by their brethren, who aver, in Langfeld's language (**23**, 89), simply that "a consistent stimulus-response behaviorism . . . has no interest in consciousness," but who do "not know what it means to deny an unmistakable phenomenon." [1] Finally, it would contradict the implicit assumptions of even the extremest behaviorist, whose pronouncements cannot but be redolent of reliance upon conscious experience, and who often indeed seems to deny the existence of other men's minds because he believes, at bottom, that other men exist only in *his* mind. It is for these reasons that the scientific logician is so astounded by the behaviorist who "finds no evidence for 'mental existences' . . . of any kind" (**42**, 2), who asserts that "'consciousness' . . . is a plain assumption" (**41**, 5), and who challenges us to 'prove it' (**43**, 7). There is not a perfect proof, but there is one more nearly perfect than any other in the entire corpus of inductive science.

If 'consciousness' means conscious experience in the concrete, the proposition "consciousness does not exist" shows itself 'absurd and impossible' by the fundamental canons of science, philosophy, and common sense. Either the proposi-

[1] Cp. to similar effect: **34**, 1; **24**, 241; **17**, 14-15. I have tried to indicate "what it means."

tion, therefore, is false, or it entails the most searching scientific revolution ever conceived, not merely in psychology but in all human concept-systems, and all logical and scientific methodology. Such revision, although not impossible, is greater than any attempted by a Plato, a Darwin, or an Einstein; its positive nature I cannot conjecture; and the behaviorists themselves have shown small interest or aptitude for it.[2] Finally, even if it were accomplished it must be so complex that no conceivable psychological advantage would warrant its substitution for the current scheme.

No living man, I think, ever seriously contemplated so recondite a possibility. How, then, can we suppose that the behaviorist was deceived into an egregious fallacy? In the first place, it is a phraseable hypothesis that, in spite of a wide prevalence of conscious experience, a few persons have none, yet are capable not only of managing their own affairs but also of correctly, if somewhat equivocally, informing us that they have none. Such a supposition, however, is as fantastic as the foregoing, since in default of profound histological differences between the behaviorist and the normal man such a discrepancy must be an unintelligible miracle, while even if the behaviorist is unconscient, he is a *lusus naturæ* whose peculiarity would not warrant a whole new psychological system. In the second place, it is more plausible that, as someone has suggested, the behaviorist suffers from an *experience-aphasia* which makes him literally unable to *notice* that he has conscious experience, just as some people are psychically deaf and as most people cannot attend to the nuances of an emotion. In the third place, the behaviorist *may* be perfectly cognizant of what we call conscious experience, 'colors, sounds, tastes, smells, and the like,' but may have supposed the introspectionist to be talking of something else, a mysterious set of 'ideas' of these elements. "Well-trained and scientifically minded persons," Woodbridge reminds us, "find consciousness unrecognizable when told that it is made up of what they see or hear" (51, 604). Discovering in his experience only the latter, the behaviorist, as Köhler

[2] An unpromising beginning was made by Weiss (46).

explains, 'derides the introspectionist for dwelling in a world of imaginary ghosts.'[3] If the behaviorist is denying only, with James (**18**, 9), that conscious experience has such 'inner duplicity,' he is denying consciousness in no sense which either introspectionist or epistemologist would any longer contest. Some theorists would maintain that the conscious content consists of 'objects,' some that it consists of 'ideas,' but only a crude confusion supposes that it consists of duplicate sets of objects and ideas alongside one another. It is possible, in the fourth place, however, that the behaviorist, although actually denying the reality of what we call consciousness, does so in some uncommon sense, out of allegiance to a metaphysical ideal, because it does not 'fit into' his philosophical scheme. "According to convention," said Democritus, the first great behaviorist rationalizer, "there are sweet and sour, by convention there is color; but in reality there are atoms and the void." The materialists of the nineteenth century employed the epithet 'epiphenomenal' to dispose of the conscious qualities which they deemed scientifically inscrutable, and the behaviorist concludes that conscious states *ought not* to exist, because they would be "isolated, unusable 'mental' curiosities" (**42**, 2). We need only describe this subterfuge, however, to show, first, that it is not permissible, because an empirical science, regardless of its metaphysical straits, cannot significantly deny the stuff which gives it its whole content and data; and second, that it is not necessary, because there are newer naturalisms than Democritus's which no longer find the conscious qualities *a priori* unintelligible (**8**, **12**, **24**).

Now, it is widely recognized that most behaviorists have intended, not to deny consciousness at all, but simply to ignore it. It might fairly be asked of them why, if this is what they meant, they did not plainly say so, and it can fairly be objected that since conscious experience is the inevitable 'starting point' of science they cannot literally ignore it *in toto* but can merely refuse to regard it introspectively, or, as the elder psychologists said, 'as such' (**21**, 1). Granted this limitation,

[3] Gestalt psychology (**20**), p. 24. The first chapter of this book is one of the best extant criticisms of the theoretical basis of behaviorism.

their choice may express a merely personal desire to specialize. On the other hand, it may express the conviction that conscious experience 'as such' is not a fit subject for any scientific attention. The more familiar and unsophisticated argument for this conviction is that conscious experience is *too peculiar* to be a subject for a science: especially, that it is private and unsharable. Somewhat less familiar and more sophisticated is the exactly opposite argument, that conscious experience is *not peculiar enough* to be a subject for a science, since it is an undistinguished part of the one physical world already parcelled among specialists. These arguments cannot of course be both maintained at once, but can either be maintained by itself?

B. Studies of consciousness are widely supposed to be unprofitable because unverifiable, and unverifiable because, as J. S. Moore explains (**27**, 138), although "*physical* or material facts . . . are *common* objects of experience for all experiencers; . . . *psychical* or mental facts . . . are *private* objects of experience for one experiencer only." Now, if this doctrine were true, one might expect such extraordinary 'intangibles and unapproachables' (**41**, 6) to challenge rather than inhibit scientific attention (**4**, 33); but it is almost certainly false. On no accredited theory of knowledge can conscious experience be more private or incommunicable than anything else. We have already seen that we do not have duplicate sets of 'observations,' one private and one public, one for psychologist and one for physicist, and we have already justified a belief in other minds and their communications. It remains to notice that in whatever sense another's consciousness is not publicly observable, in that same sense physical objects are not publicly observable.

The persuasiveness of Moore's statement, above, rests largely on our predilection to interpret 'experience' to mean *mediate* experience as it relates to physical facts and *immediate* experience as it relates to psychical facts. Now, there is just one epistemological theory which supposes that two persons can ever *immediately* experience numerically the same entity. This is the neo-realist doctrine that the conscient act is an

organic behavior-process and that the conscious content consists sometimes of 'real objects' in the environment (**15**). This doctrine, as Hunter (**16**, **17**) and Pillsbury (**28**) suggest, is peculiarly compatible with the behaviorist's faith in 'objective observation,' but it is an obvious denial of the privacy of mind. It provides, that is, for the direct and public observation, by any number of persons, either of any person's conscient act or of any person's conscious content.

More common among scientists is the dualistic belief that knowledge is indirect, that the immediately experienced consists not of parts of the objects themselves, but of nerve-processes caused by and 'representing' the objects, or of immaterial psychoses correlated with such nerve-processes. Both of these theories assert that conscious experience is 'private' in the sense that no person can be immediately aware of any part of another's conscious experience, but *ipso facto* they both assert also that no two persons can ever be *immediately* aware of *any* common object, physical or otherwise. They have therefore more frequently engendered the mentalist's doubt of the 'inaccessible' physical object than the behaviorist's doubt of the 'inaccessible' state of consciousness, so are particularly inappropriate to behaviorism. Neither doubt, however, is justified unless complete skepticism is justified. If the world is a chaos, we can know nothing; but if it is not a chaos, then by a reasonable inference from nerve-process to its objective condition, or from psychosis to nerve-process to objective condition, I can have a sufficient knowledge about physical events (such as the behavior of other people); while by an equally reasonable inference from such knowledge of other people's behavior to their nerve-processes or even their psychoses, I can have an equally sufficient knowledge about the conscious experience of other people. If the latter inference might be more difficult than the former because the line of connection is longer, it might however be easier than the former, first, because each of us enjoys an immediate experience of what intrinsically a 'consciousness' is like, as he does not, for instance, of what a buttercup is like; and second, because *language* enables a deliberate signalling from you with

regard to your conscious experience which is immeasurably superior to a buttercup's capacity to signal about itself. It makes in fine so little real difference whether we are directly experiencing the same object that nobody so far has imagined a crucial experiment to determine whether we do or not.[4]

The behaviorist's suspicion of other consciousnesses is, as Köhler says (**20**, 34), a 'one-sided and impractical purism.' The only reasonable alternative to supposing that the conscious content of other persons is, scientifically, common property, is a complete skepticism concerning not only their minds but also their bodies, their books, and their galvanometers. Such agnosticism has been accepted, albeit hesitantly and inconsistently, by recent positivists in physics and philosophy (**2**, **7**, **9**), and even by some supposed behaviorists (**33**, **46**). On the other hand, some behaviorists have striven to protect themselves not by widening the field of their skepticism but by narrowing it: specifically, by asserting not that another's experience is non-existent, or even inscrutable, but that its *qualities* are inscrutable (**25**, **38**, **39**). We can describe to one another the structure of our experience, and the mutual relations of our experiential qualities (their 'term-character,' in Tolman's phrase); but the *qualia* or 'raw feels' themselves cannot 'get across.' The *quale* you call 'red' may be my 'green' or even my 'sour.'

This highly selective skepticism, I think, although more ingenious, is even less tenable than the more thorough skepticisms. That we can know nothing is scientifically very improbable; that we can know matter but not other minds (or other minds and not matter) is much more improbable; but that we can know the structure of other minds and not their qualities is improbable to a degree which bankrupts arithmetic. It arbitrarily assumes, in the first place, a dualism of conscious content and cognitive object, without considering the common-sense conviction and the technical theory of neo-realism

[4] Philosophers have found puzzles in the notion of representative perception, that is, in any inference from a given content to an ulterior fact, and have raised special objection to the notion of inference to other minds. Their objections, I think, are groundless (**48**; **49**), but here I can only reiterate that if they prove anything they prove all knowledge impossible.

that you and I immediately perceive identically the *same* qualities. It arbitrarily assumes, in the second place, the even deeper dualism of conscious quality and physical structure, without considering the prevalent naturalistic doctrine that qualities are literally identical with their correlated 'structures' (**8, 24**). It arbitrarily assumes, in the third place, not only a dualism but an *irrational* dualism, without entertaining the almost universal supposition that qualities, whatever their dynamic connection with their structural substrata, are at least *consistently correlated* with them. In the fourth place, it assumes such irrational dualism not only in default of favorable evidence but in despite of unfavorable evidence. For (*a*) science both assumes and discovers that the world as a whole is intelligible and consistent, and there is no apparent reason why the laws of causal connection should be abrogated for qualities. (*b*) Experiment shows that these laws are *not* abrogated for qualities within the experience of any single person. A certain vibrational frequency, for instance, is always correlated for me with a certain musical tone, and does not appear unpredictably as other tones, and still less as smells or colors. (*c*) As between persons, every indication is favorable to a rational correlation. The theory itself assumes a consistent correlation of *structures* in our two bodies and two minds, and so far as I know has never suggested that so-called 'primary qualities,' such as squareness or twoness, might differ from one experience to another. Such qualities are too readily schematized. But colors, tones, and smells can be schematized too. It is hard to conceive how your color-qualities would still fit the color-pyramid if they were skewed, let us say, even 30 degrees from mine; but it seems quite impossible that what I call 'colors,' and can schematize only on the pyramid, should be what you call 'smells' and can schematize only on the odor-prism.

It might fairly be asked why, if conscious experience can in no way be more 'private' than anything else, so many gifted persons have supposed it self-evident that it *is* private. (*a*) Probably tradition, obscure religious and moral motives, and uncritical metaphysical prepossessions have had their in-

fluence. These we need not debate. (*b*) More reflectively has operated the remarkable epistemological difficulty of defining the precise relation of the conscious content to the physical world. (*c*) Many psychologists, on the other hand, seem to have meant by the 'privacy of conscious experience' merely that *introspective reports* are experimentally unreliable—that is, uncontrollably variable. The difficulty (*b*) of the psychophysical problem, however, is simply the difficulty of *choosing among* diverse epistemological theories *all* of which, as we have just seen, *deny* any peculiar unknowability of other minds (**49**), while (*c*) the difficulty of an 'introspective' report about attention or the thought-process, in contrast with an 'objective' report about the maze-errors of a white rat, cannot consist in the circumstance that the former is derived from a conscious fact and the latter is not, for both are derived from conscious facts.

I suggest that the chief reason for the 'privacy' doctrine is not that I know less about your conscious experience than about your digestion or your chemical composition, but that I want to know, and in some cases you want me to know, very much more. The precise state of your mind, especially, as Roback says (**30**), if I am in love or at law with you, seems, rightly or wrongly, much more *important* to me than the state of your digestion. I usually know much less, furthermore, concerning the state of your mind when you try to conceal it than when you try to disclose it, while even at best I know very much less of, or at least know much less intimately, the state of your mind than you do. All this is the natural and inevitable result of the fact that, philosophically speaking, each of us is immediately acquainted with, because each is identical with, one and only one metaphysical thing-in-itself, namely, his own conscious experience, and every thing-in-itself, whether a conscious field or a galvanometer, is private in that it cannot be immediately and completely absorbed into another conscious experience, or anything else not literally identical with it. Science and common sense usually do not aspire to exhaustive immediate experience of things-in-themselves, but only to indirect knowledge about them by means

of their fragmentary phenomenal appearances, particularly, in the lingo of the day, of their 'pointer-readings' (14). Contemporary physics has become acutely aware of this indirection as it pertains to the knowing of other things, but for psychology, just because each of us does enjoy the unique privilege of direct acquaintance with his own consciousness, there is special and inordinate dissatisfaction with the ordinary indirect way of 'knowing other consciousnesses.' Some of the extremer physicists meet the issue by denying the existence of 'real physical objects,' as some extremer behaviorists deny the existence of other conscious experience, while the milder physicists simply ignore the 'intrinsic reality' of ordinary physical objects, and the milder behaviorists try to ignore the conscious experience which is the intrinsic reality of other minds. We have seen, however, that indirect knowledge either of minds or of atoms is entirely feasible, more useful indeed than immediate knowledge, and that it argues for and not against the intrinsic reality of its presumed objects.

II. To ask now of the reality of consciousness in the abstract, as the universal character which defines a 'conscious' entity or the conscious field, is to ask the question, already adumbrated, whether conscious experience 'as such' may not be ignored by the psychologist, not because it is so peculiar, but because it is *not* peculiar: because it is the same sort of thing which all the sciences study, or because, as Weiss says, it will '*vanish* without a remainder into the . . . components of the behavioristic analysis' (47, vii). To deny the reality of the quality of consciousness, however, *i.e.*, to deny that conscious experience is in any respect peculiar, is to assert that conscious experience cannot be differentiated from other things. Not only we, but everybody except the Berkeleyan idealist, have assumed that some things are conscious and some are not, while even the idealist believes that there is a difference between the *conscious* character of things and their other characters. It seems likely that those who deny that consciousness as a peculiar character exists mean only, on naturalistic principles, to deny certain extreme degrees of peculiarity: that it resides in a unique, non-physical stuff, sub-

stance, or attribute, of which conscious facts are modifications; or, as James put it (**18**, 8), in a peculiar medium, menstruum, or solvent in which float the things which are 'conscious';[5] or in a simple, unique, and indescribable quality which inheres in all conscious things. Even the naturalist must suppose, however, that there is a peculiar character of consciousness which consists, like that of friendship or electromagnetism, in a relational configuration, subsisting either among the conscious elements or among the conscious elements and a conscient mind, organism, or nervous system. This 'relational theory of consciousness' is a recent favorite among both philosophers and psychologists (**8**, **12**, **15**, **18**, **24**, etc.), and is consonant with the program of natural science, which would show all differences as specifications of a single mensurable continuum. It is not a denial that consciousness is unique, any more than atomic analysis is a denial that saltpetre is unique, and looks like a denial only to persons who feel that consciousness must be extremely and 'metaphysically' unique. A scientific psychology might reasonably be expected, not to ignore the rather striking phenomenon of consciousness, nor even to cause it to 'vanish without a remainder,' but to submit it to analytic research.

III. Turning finally to some notice of what we call 'consciency,' we might expect a parallel ambiguity to that of 'consciousness.' 'Consciency,' that is, might refer to the concrete class of agents, acts, or processes which are conscient. Actually, however, all scientifically significant statements about consciency with which I am familiar refer to it only as the abstract character which defines the class.

Since consciency is by definition the attribute of the living process, whatever it may be, which conditions the occurrence of consciousness in the content, it is impossible to admit the reality of consciousness without admitting the reality of consciency, unless one supposes that consciousness has no conditions either in a bodily or in a psychical 'subject.' The latter may be the contention of Hume's associationism, according to which ideas and impressions combine of their

[5] It was primarily in this sense that he *denied* consciousness.

own forces, and of the behaviorism of Weiss (**47**) and Lashley (**24**, 341), who seem to hold that the conscious content is literally identical with what would otherwise be called the conscient response. Among those, on the other hand, who consider that consciency is to be distinguished from consciousness, consciency is variously understood as the non-natural act of an immaterial mind or faculty (as by McDougall and the older 'intentional' psychology), a non-natural act of the material organism (as it is, apparently, by some recent British realism), or as a natural act (or response) of the material organism (as it is by some American realism and by philosophical behaviorism). There is no obvious reason why a scientific psychology ought not discover and analyze the character of consciency, and Carr (**10**) and Tolman (**37**, **39**) are among recent psychologists who recognize as much.

Summary

The object of this paper has been to inquire not so much into the answer to as into the meaning of recent ambiguous disputes concerning the reality or scientific significance of 'consciousness.' I have submitted:

1. If 'consciousness' means conscious experience in the mass, (*a*) its reality can not significantly be denied in any explicit conceptual system so far known to common sense, science, or philosophy, and (*b*) the popular belief that it is peculiarly private or unknowable is contradicted by every extant theory of knowledge and especially by behavioristic realism.

2. If 'consciousness' means the abstract character which differentiates those things which are content of conscious experience from those which are not, (*a*) it may not be supposed to be non-existent without denying the differentiation, but (*b*) it may be supposed to be a natural or structural peculiarity and not, as in traditional dualism, a substantial or metaphysical peculiarity.

3. If 'consciousness' means *consciency*, the character of the organic or psychic process which conditions the occurrence of conscious content, (*a*) it may not be supposed to be non-

existent without denying either that consciousness exists (in the second sense, above) or that it has conditions, but (*b*) it may be supposed to be a natural and analyzable function of the animal subject.

These conclusions do not depend upon the acceptance of any particular philosophical hypothesis, but only, I have tried to show, upon the avoidance of a skepticism which would make all science and discourse impossible. Without deciding definitively in favor of either the behavioral or the introspective method of psychological observation, we have found a basis upon which can be discussed their nature and relative merits. If the behaviorist must philosophize, he should first of all be clear that the admission of consciousness does *not* imply the reliability of the introspective method, that neither consciousness nor introspection implies psychophysical dualism, that dualism does not imply the existence of a substantial soul, and that even the existence of a substantial soul does not imply God, freedom, and immortality, or the prerogatives of a sacerdotal class.

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[MS. received March 2, 1934]

SOME EARLY BEHAVIOR PATTERNS IN THE WHITE RAT

BY AMOS C. ANDERSON AND JAMES R. PATRICK

Ohio University

Introduction

In view of the current interest in the problem of the genesis of behavior the writers thought it worth while to make some observations on the early reflexes and the early appearance of behavior, reflex or otherwise, in the rat. The problem as first conceived was simply to determine whether certain of the obvious behavior patterns of the animal, such as starting to a loud noise, usually assumed to be reflex, were present at birth; and if not, whether they appeared suddenly at a given age, or appeared gradually. As the observations progressed we became interested in certain other phases of the early behavior of the animals, suggested by the unfolding of the animals' behavior repertory. One problem which especially engaged our attention was whether certain of the early reflexes disappeared (waned) at a given age, or lost their identity somewhat within larger behavior patterns. Another problem was to ascertain what evidence there is that specific reflexes, or other behavior patterns appearing subsequent to birth, represent a process of 'individuation,' to speak in terms of behavior, or 'canalization,' to speak in neural terms, out of larger and more diffuse patterns. While it is not our intention to give here a summary of the work done to date upon the subject of behavior genesis, we feel obliged to mention the work of Coghill (**1**, **2**), Angulo y Gonzales (**4**, **5**), Kuo (**7**), and the Shermans (**8**) as having in a measure stimulated our own interest and influenced our interpretation of our results. For additional references on related topics, especially fœtal behavior in various species, the reader is referred to Angulo y Gonzales (**5**) and Kuo (**7**), and for observations on geotropic orientation to Crozier and Pincus (**3**).

General Method

Observations were begun the day after our discovery of each of the litters used. As the colony was fed but once a day, usually in the late morning or in the afternoon, we did not know whether a litter had been born the day we found it, or the latter part of the preceding day. We dated the rats' birth as of the day we found them. Thus the fifth day, as used below, means the fifth day of the animal's life, assuming its birth to have occurred on the day we found the litter. Forty-five animals were observed, divided among eight litters. They were from our experimental colony of albino rats, originally Wistar Institute stock. With but one exception, mentioned in its proper connection, the animals were tested one at a time. Each animal was placed upon a piece of paper on a table, and given the various stimuli, except for such tests as necessitated holding the animal in the hand, dropping it or placing it on a screen. Except for some preliminary observations made by one of us alone, each of the writers administered the stimuli for some litters and recorded observations for others while both observed. While we tried to employ the same technique, differences in our handling of the animals and presentation of the stimuli seemed to be unavoidable. We found essentially the same results, however.

Stimuli, Results, and Discussion

Front grasp.—The stimulus for this reflex was pressure upon the soles of the feet produced with the shaft of a common pin. The rat was held in the hand with its feet free to swing. A firm gentle pressure was exerted. The rat responds to this pressure, at least in the first few days of life, by bending the toes downward as if to grasp the object. At this early stage the toes are so short that the animal is unable to encircle or grasp the pin, but the toes are frequently bent down enough to diminish their blood supply causing them to whiten. It was found that pressure at the juncture of toes and sole of foot elicits the grasping reflex with a high degree of regularity. Pressure on the 'palms' seems to produce it slightly less

regularly. At times there seems to be a summation effect, pressure on the 'palm' eliciting a weak grasp or none at first, but a clear cut response with further stimulation. Sometimes the animal presses upon the stimulus object without actually grasping. In general, however, the grasp with the front foot, beginning at least as early as the second day after birth and probably earlier, is one of the most consistently found bits of behavior in the white rat at this age. Angulo y Gonzales (**5**) reports such behavior in the nineteen-day rat fœtus. He states, "Stimulation of the dorsal side of the hand always causes hand extension, and stimulation of the volar side, hand flexion." Our notes for a group of eight rats, one day old, follow. The stimulus was, in all cases, pressure produced by the shaft of a common pin.

Rat 1. Grasp occurred when touched on sole, not much when touched farther up toward ankle.

Rat 2. No grasp when touched at juncture of leg and foot, or on pads of foot. Clear grasp when touched at juncture of toes and foot.

Rat 3. Slight grasp when stimulus was applied on sole, strong grasp (toes whitened) when applied at juncture of toes and foot.

Rat 4. Clear cut grasp when stimulated on volar surface of left foot; less clear when stimulated on volar surface of right foot except when stimulated at juncture of toes and foot, then clear.

Rat 5. Left foot showed pronounced grasp when stimulated on sole, but right foot only slight grasp when stimulated in same region. Pronounced grasp with both feet when stimulated at juncture of toes and feet.

Rat 6. Clear grasp on each foot when stimulated at sole; only slightly stronger when stimulated at juncture.

Rat 7. Grasp occurred when stimulus was applied to sole; stronger grasp to stimulus applied at juncture.

Rat 8. When stimulated on sole, pushed and bent toes slowly. When stimulated at juncture curled toes with less pressure on pin.

The third day after birth such notes as these appear:

Rat 1. Slight pressure forward when stimulated on pads. Clear grasp occurred when pressed at base of toes.

Rat. 5. Pushed against pin when stimulated at sole, slight closure. Grasped pin when stimulated at base of toes.

Notes by the end of the first week indicate an increasing tendency to push against the stimulus, especially when applied to the pads of the feet. Although the grasp still appears at the end of the first week, and may occur in its original strength when the stimulus is applied at the juncture of toes and foot, as a rule it occurs less regularly. The eighth day we read:

Weak grasp. Less difference in the strength of grasp when stimulus was applied at sole and at juncture. The rat's foot seemed to push forward more than to grasp.

From here on we observe a change in the character of the grasp as is shown in the following notes selected at random during the observation period between the ninth and fourteenth days.

Pushed forward and grasped. Not like old grasping reflex but more of a push with toes curling.

Pushed and grasped stimulus as if for support.

Grasped and hung on to stimulus.

The presence of the grasping reflex in the infant rat with its subsequent 'waning,' or, as we believe, its incorporation within a more inclusive behavior pattern, is of singular interest. It has long been observed in human infants, and its use by students of evolution as an evidence of arboreal ancestry is too well known to require comment here. Holt (**6**), on the other hand, explains it in humans in terms of the reflex-circle. "In the fœtal position the fingers are often closed over the palm of the hand, and the least random flexion of a finger will cause it to press on the palm. Then (what is not random) afferent impulses (tactile) from the two surfaces in contact (palm and finger) will be sent back to the central nervous system, where by the principle already cited they will find an outlet in the motor paths that were just now excited, that is, those of the flexor muscle of the finger in question. When this has happened a few times (as it is bound to happen) the reflex-circle will be established;

and then a pressure stimulus on either palm or finger will cause the finger to flex and so to close down on the object that caused the pressure. Such is the origin of the 'grasping reflex' which is so useful through all the later life. This reflex is regularly established before birth."

The presence of this reflex in the rat does not, of course, disprove either theory of its human origin. It seems improbable that it ever played the part in the rat that evolution assigns to it in the human. As to its reflex-circle origin it may be pointed out that the rat's front toes are so short at birth that they could hardly rest closed upon the sole, much less the juncture of toes and foot, in the fœtal state. Indeed, in the first few days of life the rat bends the toes down but does not close the front foot, the toes being too short as even the most cursory examination will show. The trait may have had one origin in the human and quite another one in the rat. But its appearance in the rat at least suggests the inadequacy of either the theory of 'arboreal ancestry' or the 'reflex-circle' to account for it in the human. It may be that the use of the hand in grasping, being highly useful to the adult primate and rodent alike, has, because of its survival value, become fixed in both forms of animal life as a behavior pattern, the nervous mechanism of which is established previous to birth. This is no more teleological, if we must in the interest of academic respectability avoid teleology, than to admit that the vast bulk of our vegetative reflexes, *e.g.* breathing and peristalsis, have their nervous mechanisms established early. Coghill's work with amblystoma shows that certain essential behavior patterns are established early, and he details relation between structure and function. Such a view as we here suggest would seem to be more in accord with the results of recent research.

While the grasping reflex appears early in the life of the white rat, it seems to wane in the second week, and we failed to find it in any thing like its earlier form in the adult rat. Day to day observation, however, casts some doubt upon its being a pure case of waning. On the second day we found an occasional rat pressing toward the stimulus as well

as curling the toes downward. This occurs with increasing frequency, until in the second week the grasp is fairly regularly accompanied by pressure of the foot on the stimulus, and if the stimulus be slowly removed the foot often follows it. One group, not the one mentioned above, on the twelfth day showed such responses as these:

Rat 1. Grasped only when he moved or support was removed.
Rat 2. No grasp.
Rat 3. No grasp.

In the early stages of waning, the grasp is accompanied by a pushing out toward the stimulus. In later stages of waning when simple pressure no longer elicits the grasp, removal of support (allowing the animal to start to fall) causes the grasp to appear in full strength. By the time (twelfth day) the behavior just described occurs, the front foot has developed greatly, and the animal is able to use it for support, or at least to check a fall. Here, it seems, we find a case of an early reflex pattern losing its identity in a larger and more complex behavior pattern, *i.e.* in the general use of the front foot. The simple stimulus originally eliciting the grasp is no longer adequate, but other stimuli become adequate to bring it out either alone or as part of more complex behavior.

Although this reflex appears so regularly and is so clean cut that it seems inconceivable to us that anyone exploring for it should fail to find it, we realize that some persons may regard the flexion of the toes as simply a mechanical effect of the pressure of the stimulus without being a reflex at all. But we have found even the lightest pressure to elicit the grasp reflex. Accompanying the grasp the foot may reach forward as the stimulus is slowly withdrawn. We can only interpret these characteristics of the grasp as responses to, not mechanical effects of, the stimulus. Further, were it merely a mechanical effect, it should appear in the hind feet as well as in the front, but this we did not find to be true. Its appearance in the hind feet was slight and irregular.

Rear grasp.—The hind feet were stimulated for the grasp in the same way as the front. While grasping behavior

appears at times, it is much less in evidence than in the front feet. Notes for the second day after birth read as follows:

Rat 1. One slight grasp when stimulus was applied at break of thumb. Stimulus repeated and grasp occurred again.
Rat 2. Slight grasp to stimulus applied at toe juncture. Appeared twice out of many stimulations.
Rat 3. Grasp appeared once out of (roughly) a dozen stimulations at the juncture.
Rat 4. No grasp though the stimulus was applied many times.
Rat 5. Slight grasp, obtained only rarely at the juncture though stimulated many times.
Rat 6. Slight grasp.
Rat 7. No grasp, though many stimulations.
Rat 8. Toes, when pressed in juncture, bent slightly.

This primitive grasp in the rear foot seems to occur when only a limited area is stimulated. It appears at times fairly definitely as a response to pressure at the juncture of toes and foot, but scarcely at all for pressure on the pads of the feet. Even when it appears, which may be only after many stimulations, it is weak, and seldom leads to a whitening in the toes. Like the front grasp it is often accompanied by a pushing toward the stimulus. The presence of the rear grasp was not noted at all until fairly late in our observations. In fact, one of us tested for it more or less unsystematically in at least two litters without noticing it. Upon systematically testing for it daily, one of us administering the stimuli and the other recording while both observed, it was noted to the degree described above.

Retraction.—While holding the animal in the hand (its feet swinging free), the head of a pin was drawn lightly over the sole of the hind foot, in the direction from heel to toe. Often merely touching the sole was adequate. The response was a prompt withdrawal or retraction of the foot similar to the response made by the human organism when tickled upon the sole of the foot. Retraction appears in the rear foot with about the same regularity as the grasp in the front foot. It is much more commonly found than the rear foot grasp. In one group of eight rats it appeared in one or both rear

feet in every case on the second day. It does not always appear upon the first stimulation, nor does it always appear in both feet. But it usually appears after one or a few stimulations in either foot, and when it fails, may usually be elicited by giving another stimulation or two a few moments later. When the sole of the hind foot is lightly pressed instead of being stroked, retraction occurs frequently. When we stimulated for the grasp, we often got retraction. Pressure upon the sole elicits retraction more often than it does the grasp, but pressure at the toe juncture is more frequently followed by the grasp. Such irregularities as failure of retraction to appear at first stimulation, although it appeared later and in turn failed again, or failure to appear in one foot although it appeared in the other, we believe may be attributed in large measure to imperfections in technique. It is especially to be noted that if the rat is doing something else, squirming, shivering, or kicking, these early behavior patterns are likely to fail to appear. As the animal grows older and comes to use its legs more, mere pressure upon the sole is more likely to be followed by a reaching out toward or pushing against the stimulus as if for support. But retraction may appear at any time, at least in the first three weeks or month of life although with decreasing frequency. What may appear to be a waning of the tendency seems rather to be an obscuring of it by other and perhaps more important behavior which comes as the animal matures and becomes capable of greater activity. Retraction seems to decline as kicking out, pushing toward the stimulus, or reaching out and grasping develops in the rear feet and legs. In general we may say that on about the eighth to tenth day reaching out for support supplants the retraction to a marked degree.

Righting on screen.—The animals were placed one at a time upon a screen, held at an angle of about thirty degrees. This was ordinary wire window screen tacked to a frame. From the very first we found that they 'hold' on the screen with the toes. When placed on the screen head down, the head and tail tend to be held well up away from the screen, but when placed on the screen head up, the head and tail are

commonly held close to the screen. When in the head down position, even as early as the second day of life, the head is often swung from side to side as if the animal were trying to turn around. The head is often lifted high, which has the effect of reducing the amount of slope of the body. By the fifth day the animals consistently attempt to reverse. They swing their heads sideward, change their footing, and occasionally get to a horizontal or even to a reversed position. This tendency to right when on the inclined plane seems to be present from the first and manifests itself in the form of uneasiness when inverted, as is shown by the swinging of the head. The animal when placed head down upon a screen begins to change footing as soon as it is mature enough, and complete righting awaits only sufficient maturation (strength or coördination or both). Righting persists, it seems, throughout life, and is so uniform in its appearance that we have for years demonstrated it, among other bits of animal behavior, to our classes.

Righting when dropped.—The animals were held in a horizontal position, back down, and dropped from a height of about a foot, landing upon a pad of cotton. At first they regularly land upon their backs, showing no tendency to turn over while falling. By the eighth day we found them still usually landing on the back but turning over immediately after landing. By the thirteenth day they land on the side frequently, occasionally on the feet or belly, still on the back frequently. But if they land upon the back or side they roll over so quickly that at times it is actually hard to be sure how they landed. Of one our notes read: "landed on back and rolled over. Second time, on side and rolled over. Animal seemed to bound up after landing and fall back on feet." Our notes contain such expressions as 'springs over,' 'flops over at once.' On the sixteenth day a note at the end of our day's records states that all of one litter of eight seemed to definitely attempt righting. It is to be noted however, that even this late some failed, landing on the back or side. On the twentieth day, all eight rats of this litter were dropped twice, with but three landings on the back out of the sixteen

trials. The next day all landed on their feet at least once out of two trials per animal, and on the feet or side the other time. In another litter of eight, complete righting occurred in all by the twentieth day. We cannot be sure just when righting begins for two reasons. There is always the possibility that some of the animals were so dropped as to cause them to roll over as they fell. But since they usually landed on the back at first, and as they grew older landed more and more often on the feet and side, we may assume that there is some other factor at work besides any possible imperfections in our technique. The other difficulty is to be sure just how they really do land. For several days before complete and regular righting took place, the animals landing on their backs or sides got onto their feet so quickly that we could not be quite sure that they did not land that way. Complete righting, occurring uniformly, seems to come at about the time of the opening of the eyes. Long before the animal is able to right while falling, it attempts to right when held inverted in the hand. If held by the hind quarters, inverted, the animal twists its body so as to get the head and thorax as nearly as possible into the normal upright position. We did not test for this regularly but noticed it at least as early as the tenth day.

In righting we have another example of a trait which appears in rudimentary form very early. Its complete expression is a matter of gradual development. In the first few days of life the animal is able to perform only the most elementary righting response, rolling from back to belly. A little later it is able to right upon a screen, as described above, and shortly thereafter makes some attempt, however unsuccessful, to right while falling. This latter response is gradually perfected, appearing fairly uniformly about the twentieth day.

Start reflex.—Two stimuli, noise and blowing a puff of air upon the animal, were used for this reflex. The animal was placed upon a table and an ordinary snap mousetrap was sprung over its head. The same stimulus was presented while the animal was held in the hand. Again, while the

animal was in the two positions just described, the experimenter blew a puff of air against it. In some of the later litters these stimuli were applied to the animals in the nest en masse in the first days of life, the animals being tested individually after the response appeared.

To these stimuli the animal responds with a start or jump, which can be seen easily with the unaided eye and can be sensed tactually when the animal is held in the hand. It appears first for the puff of air, about the fifth day or shortly thereafter. Not until the eleventh day or thereafter does it appear for the noise. It seems to appear fairly suddenly, not infrequently being absent one day, and clearly noticeable the next. We are far from certain, however, that it really comes with any great degree of suddenness. We have consistently observed that, when the rat is held in the hand with its feet allowed to swing free, there is a quick forward or upward movement of the hind feet accompanying, or occurring as a part of, the start when the trap is sprung. This not infrequently occurs a day or two before any actual start in the sense of a general bodily movement is observable. We have even thought at times that we could observe an increased visceral disturbance, easily observable in the very young animal, immediately following the springing of the trap. This is a very uncertain point, however, as these visceral disturbances come with great frequency and some considerable irregularity. Indeed, it is highly probable that the animals simply do not hear in the days immediately following birth. We hope soon to test for the beginning of hearing by some form of the conditioning technique. Certainly the start reflex comes to its full development only after several days of post-natal life, and does not show, so far as can be detected by ordinary observation, the steady progression in its development that is so clearly observable in the righting response.

Pain stimuli applied to various points.—In order to get some further idea of the earliest responses, and how they become modified, we administered pain stimuli to the nose, the folds of skin behind the front legs, the front and hind feet,

tail, and occasionally to the thorax, back and abdomen. The stimuli, which varied somewhat from litter to litter, consisted of light prodding with the point of a pin and pinching by pressing down on the tops of the feet with the shaft of a pin. Typical of these observations are the following in which one rat out of a litter of three at the age of four days was prodded with a small nail, less sharp than a pin.

Pricking nose region: withdrawal.

Pricking tops of hind feet, the animal resting on the table: body bent toward foot pricked, then other way, flopped back and forth for a few seconds.

Prodding belly: hind feet drew up, especially noticed first on side prodded.

Prodding chest: both front feet came down toward place stimulated, one foot rested on nail.

Rather regularly for a prick or severe pressure on top of the front foot the animal lunges forward, swinging the rear end of the body toward the foot stimulated. Similar stimuli upon the hind foot are followed by a bending of the front end of the body toward the foot pricked. For one litter of four, tested the second day, our notes read: "Rolled body toward or turned toward the hind foot pricked. One animal turned away from pricked foot, all others toward." We used the word 'rolled' because the animals often rolled over in responding to this stimulus. Similarly for another group of seven, the tendency was to turn toward the hind foot pricked. It is also to be noticed that after a fairly strong pain stimulus has been administered to the top of the hind foot, the foot is slowly raised from the table, the weight resting upon the lower surface of the leg.

In general it may be observed that the early response to pain stimuli, pricks or pinching of feet, nose, skin folds under shoulders, sides, etc. is a gross bodily movement, the rat often rolling clear over when a foot is pinched or a side prodded. With maturation this becomes somewhat more specific, the response coming to be more limited to the part stimulated. Thus early in life a prick on the hind foot may bring about a 'whole body movement,' but later a 'whole

leg movement.' This individuation of the response is only partial at best, since even in the first days of life the body tends to bend *toward* the side stimulated. Even at the beginning of post-natal life, the animal responds, we believe, with at least some reference to the locus of stimulation and to the type of stimulus. On the other hand, even late in life, intense pain stimuli may bring about chaotic responses. Coghill (**2**) stresses the importance of the individuation of specific out of larger or more diffuse behavior patterns. Angulo y Gonzales (**5**) is convinced that in the rat at least "individuation seems not to be achieved through disintegration or breaking up of the total patterns, but by an inhibitory process whereby total patterns tend to go into the background, where they remain in a seemingly dormant stage, but from which they may be aroused at any time later upon proper stimulation."

It is to be remembered that we are reporting post-natal behavior. While we see here a continuation of the individuation process begun before birth, and in other behavior patterns (as righting) see it almost from the beginning, and while we are sympathetic with the interpretation of Angulo y Gonzales, we must also emphasize the synthetic side of behavior. Pre-natal life may be presumed to be largely taken up with the development of specificity of behavior patterns in the sense emphasized by the writers quoted above. This continues well into post-natal life. But we are no less impressed by the reorganization, the recombining, the interrelation of these more or less specific patterns in post-natal life. Adaptation may be in the direction of achieving further individuation, but it may also be in just the opposite direction, a combining or synthesizing process. *That is to say, individuation may occur in the sense of a dropping out of parts of the mass activity not involved with regard to the locus of the stimulus applied, but a synthesis and reorganization of activity of parts to which the stimulus is applied seems also to take place.* We should not think of birth as a point where one process leaves off and the other begins. We see the individuation process carrying on long after birth. But we also see certain

rather definite reactions losing their identity in organizations of larger wholes; or else new elements of behavior appearing while others are dropping out. We are indebted largely to Kuo for the conception that what we call the synthetic processes may begin well before birth.

Summary and Conclusions

Observations were made by one or both of us on the reflexes or behavior patterns described above. We used eight litters of rats, consisting in all of forty-five animals. The behavior patterns mentioned in our summary are to be understood as being present from the first unless otherwise indicated.

1. The animals respond to pressure on the bottom of the front foot, especially at the juncture of toes with foot, by a downward movement of the toes which we call 'grasping' although the toes are too short at first to permit a true grasp. This primitive type of grasp seems to wane in the second and third weeks, but we believe that a better interpretation is that it becomes incorporated in a larger behavior pattern.

2. To pressure at juncture of toes and foot in the hind foot, the response is weak grasping usually, retraction occasionally.

3. To pressure upon the sole of the hind foot, the response is retraction usually, weak grasping occasionally.

4. Placed upon a screen held at an angle of about thirty degrees, the rat holds on, whether in an upright or inverted position (head up or head down). But it assumes a different posture for the two positions, raising the head when in the head down position, thereby keeping the head more nearly in the same plane as the body. From about the fifth day on, when placed head down upon a screen there is a definite attempt to gain a reversed position, resulting increasingly in success until found uniformly (for one litter) by the seventh day, the response being made more quickly and with less effort as the animal matures.

5. If the animal is held in an inverted (dorsal side down) position and dropped, it at first lands upon its back, but gradually perfects the righting response until it appears fairly

regularly by the time the eyes open. This time varied in four of our litters from the fifteenth to the nineteenth day. All members of one litter mentioned above righted when dropped the twentieth day, which was the day after the eyes opened.

6. When a puff of air is blown upon the rat it 'starts,' this behavior being observed the fifth day in some animals and shortly thereafter in others.

7. When a sharp sudden noise is made near the rat, as by springing a mousetrap above its head, there is a start. This was never observed before the eleventh day, and for most of the litters appeared on the thirteenth or fourteenth day.

8. Pain stimuli applied to the feet and sides tends to be responded to by flexing the body so that its ends, or free end in the case of foot stimuli, bend toward the point of stimulation. The response to pain stimuli tends to become more specific and less chaotic, with maturation.

These observations, we believe, warrant the following conclusions.

1. Certain reflexes as grasping and retraction, which are present early in life, seem to wane, but this waning is at least to some degree apparent rather than real. The early grasping reaction loses its identity in a larger and more significant behavior pattern. In the case of retraction, actual waning may occur as in other reflexes, but we believe that the reflex is obscured by the tendency of the animal to do one thing at a time, since it often fails to appear when the animal is engaged in some other activity. As the animal becomes more active, a stimulation for any given reflex is increasingly likely to occur at such a moment as to be in conflict with other activity. The increasing use of the foot in pushing, kicking, and seeking support, we believe, especially obscures or supplants the retraction.

2. Other reflexes as those made to pain stimuli do not appear to wane, but do show a modification in the form of more adaptive responses. Such changes are in the direction of greater precision of response, a more restricted portion of the body moving in a more adaptive way. They become less diffuse, more individuated.

3. It is the more precise type of reflex, *e.g.* grasping, retraction, and sucking movements (the latter observed but not described above) that seems to wane or become synthesized with other behavior and so obscured. The more diffuse or whole body movements, however, seem to be modified as described under 2 above.

4. Righting, presumably a reflex response, appears in a rudimentary form early in life and shows a gradual development until with sufficient maturation it reaches its full development.

5. Starting to a sudden puff of air or a sudden noise seems to appear rather suddenly, to develop quickly, and to persist throughout life. We are far from sure, however, of the suddenness of its appearance.

6. The presence of the grasp reflex in the infant white rat demonstrates, we believe, the inadequacy of both the *arboreal ancestry* and *reflex-circle* explanations of its presence in the human infant, though it does not render either of them untenable. Indeed, neither of these explanations has been set forth primarily to *explain* the grasp. Rather have they been applied to the primitive human grasp largely by way of illustration or additional proof of each of the two theories respectively. Thus they become little more than cases of making use of the human grasp as a form of special pleading for each theory in turn. The presence of this bit of behavior in the rat suggests that it has become fixed in both rat and human in the same way that other behavior, necessary for the continuance of a species, became fixed. This, we are aware, is not an explanation, but we are better content with a mere restatement of the fact than with an explanation which can apply to but one species of two in which the trait to be explained appears.

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*Although no mention is made in the text to references 9 and 10, the writers feel obliged to include them here: reference 9, because of certain observations closely related to our own; reference 10, because of its historical significance.

[M.S. received May 9, 1934]

NEUROSIS AS LEARNED BEHAVIOR

BY RALPH M. STOGDILL

Ohio State University

It is the purpose of this paper to present a generalized definition of neurotic behavior in terms which will lend themselves to objective verification both by controlled experimentation and by clinical observation. In order to be of any clinical value, a definition must describe the phenomena to be isolated with such accuracy that it may serve as an effective criterion for clinical diagnosis. From the theoretical side, it should suggest the basis upon which experimentation might be founded, and the direction such experimentation should take in the verification or invalidation of the definition itself.

Of the current conceptions of neurosis, it should be noted that the psychoanalytic definition fails to satisfy the second criterion proposed. Although a large mass of clinical data has been accumulated under the psychoanalytic body of doctrine, this system has been very sterile in suggesting anything in the way of experimentation or scientific validation. At the other extreme, the neuro-pathological conception seems to have contributed nothing but misunderstanding in many circles where sound scientific conceptions of human behavior are extremely important. The pathologist has been able to show no more justification for explaining neurotic behavior on the basis of some hypothetical organic lesion than could be shown for such an explanation of any of the other learned reactions like walking, reading or writing. (While denying that neurosis may be due to a lesion, we are by no means denying that it is solidly based on organic structures.) The latter criticism applies also to those theories which attempt to explain neurosis on the basis of defective inheritance, endocrine imbalance, fatigue or depleted nerve energy. Although the presence of one or more of these

factors may seriously affect the learning efficiency of the organism, it is difficult to conceive how any of them could determine *what* is learned. If defective inheritance, fatigue, and the like are factors in the acquisition of neurotic behavior it is in all probability because they incapacitate the individual for warding off the disorganizing onslaughts of the environment.

I. The Fortuitous Conditioning of a Compulsive Gesture

The following case is presented as an example of what might easily have been mistaken for a compulsion neurosis had it not in an early stage of development been interpreted from a strictly objective point of view. A laboratory assistant was observed to approach with extreme caution the steel filing cabinet into which he was transferring supplies. Each time he reached out his hand to open one of the steel drawers he drew it back rather hastily, then struck the palm of his hand firmly on the table, against the wall, or on his person before finally touching the drawer to pull it open. He picked up his supplies rather gingerly and approached other objects in the room with caution when contact with his hands was required. Incompleted gestures of approach and withdrawal, and occasional random gestures were observed. To the observer the reaction fulfilled externally all the requirements of a compulsive gesture such as characterize compulsion neuroses. It appeared repeatedly, automatically, and without any apparently relevant stimulus.

Upon being asked the reason for his unusual behavior, the assistant explained that in walking over the carpeted floor he accumulated a charge of electricity which was quite noticeably discharged when his fingers came in contact with any object, especially any metal object. He added, that by slapping his hands firmly on his own body or on some other object the shock was less noticeable. The writer, by shuffling his feet over the carpet, and then touching the filing cabinet was able to verify the fact that an unpleasantly noticeable electric shock was obtained.

It was observed that the assistant's gestures of withdrawal from contact with objects in his vicinity ceased upon his leaving the building in the evening. The behavior was not noticed anywhere except in the two rooms where the shocks were experienced. This was corroborated by the assistant's observation of his own behavior. Upon reentering the room with the filing cabinet, however, the gestures of slapping or pounding before touching an object continued, often when there was very little likelihood of his receiving a shock.

No use was made of the interview technique or dream analysis to ascertain whether or not the gesture was symbolic of the casting out of repressed incestuous desires, or of any erasure of disgusting memories of childhood indulgences. The writer is unwilling to deny the possibility that the gesture might attain such irrelevant significance through the proper juxtaposition of conditioning circumstances. When considered, however, in the light of the definition to be proposed, it must be denied any status as a neurotic form of behavior.

II. A Definition of Neurotic Behavior

A functional neurosis is to be regarded as (1) a disorganized or emotional form of behavior (as estimated in terms of the social criterion: degree of adjustment to environment), and (2) a response conditioned to, or redintegrated by, a remote and irrelevant stimulus. It must not be assumed that all emotional or disintegrated behavior is neurotic; nor is all behavior conditioned to a remote stimulus to be regarded as such. The disorganized (emotional) response is one of the normal emergency reactions of the human organism. It is regarded as neurotic only when it occurs in a non-emergency situation, or is expressed in an inappropriate manner in a truly emergency situation.

Again, the redintegration of responses by succeedingly remote and curtailed aspects of previous stimuli or stimulus situations is regarded as the normal process of symbolic behavior. Innumerable responses of everyday life are elicited by stimuli so abstract (far removed from the original by successive conditioning) as to escape casual analysis. Thus

arises the quandry of the neurotic patient who finds himself committing acts for which he has no explanation—acts which, although appropriate to the situation in which they first occurred, now seem bizarre and beyond control. The inadequacy of the psychoanalytic conceptions of repression and the unconscious now becomes apparent. The fundamental purpose served by these concepts was that of explaining why any given form of abnormal behavior might 'lie dormant,' without being exhibited over long periods of time, and then suddenly be reactivated without any apparent cause. The psychoanalytic doctrine assumed that this persistence and repetition of abnormal acts was due to the activity of energized ideas repressed into the unconscious. The present conception takes the stimulating environment into consideration, as well as the organism, and substitutes for 'repressed ideas,' the conditioned or learned response as the necessary mechanism for the repetition of 'abnormal acts,' just as for normal behavior.

In terms of our definition then, is the gesture described in the previous section to be considered neurotic? The response is clearly a learned form of behavior. A certain amount of disorganization or emotion was indicated to the observer by the slapping, pounding and random gestures of the subject. Thus, the first condition was, to a certain extent, fulfilled. However, these responses, although apparently inadequate, were not found to be irrelevant. They were not introduced into the situation by previous *social* conditioning and did not possess any extrinsic symbolic significance. Thus the second criterion was not fulfilled. The stimulus, an electric shock, constituted an immediately relevant and adequate cause for the behavior observed.

Had the original conditioning situation been primarily of a social nature the chances of dissociation (response to remote and irrelevant stimuli) would have been multiplied manyfold, as the psychoanalysts have pointed out. For example, had some other observer been in the room with the assistant, and tickled him, teased him, or otherwise humiliated him whenever he exhibited such behavior, it is not incon-

ceivable that he might have become conditioned to jerking or waving his arms whenever he met this observer in other situations, especially where attempts were made to tease him. Other individuals, recognizing the fun that might be had at the assistant's expense, might have joined in and become further stimuli to such reactions. It is not unusual to find that individuals who have been thus conditioned, jump, cringe, grimace or gesture when approached by any other human being. In fact, inferiority reactions of all degrees of seriousness and in all stages of development may be observed in the making where any group of people are engaged in social intercourse.

The response under discussion must also be denied any status as a neurosis according to the psychoanalytic conception of a neurotic complex as 'an emotionally toned group of ideas subjected to repression.' Disregarding the subjective implications of the term 'ideas' in this definition (which can be done, the writer believes, without sacrificing anything of value in the study of neurosis) we observe that, although the factor of '*emotion*' or disorganization does, in some degree, apply, the factor of '*repression*' does not.

III. A Learned Disorganization of Behavior

A clinical case may be presented as an example of the learning of actually neurotic behavior. A student, on his way to the campus with a group of classmates, stepped from the sidewalk in front of a rapidly approaching taxicab. He was greatly startled by the squeaking of the brakes, the sound of the horn, and the sight of the swerving car bearing down upon him. He jumped back violently to escape being hit. The taxicab driver was loud and exhaustive in his characterization of the student's mental incompetency. The student's companions, taking full advantage of a highly amusing situation, joined in with the taxicab driver, and kidded the student about his pale face, trembling knees, and 'wool gathering.' The student was 'struck helpless' with humiliation, and filled with 'voiceless rage' at the taxicab driver, at his companions, and at the group of giggling women

acquaintances. He left the scene without a word, and went to the corner drug store for a bromo seltzer to 'settle' his stomach. He floundered badly in his recitations that day because he was preoccupied with delivering, subvocally, violent and scathing replies to the abusive remarks of the taxicab driver and the kidding of his companions. In his implicit reactions, he 'beat up the cab driver' in revenge and thus demonstrated his manhood before the group of women students.

The next morning, on his way to the campus, a student called after him, "Look both ways before you cross the street." He made no *audible* reply. On succeeding mornings, however, he walked to the campus alone, several minutes before his previous companions started. He avoided anyone whom he thought knew about the incident.

A few weeks later, the student noticed that his stomach was becoming 'badly upset.' His family physician diagnosed the difficulty as dyspepsia, due to improper diet, irregular elimination and over-work. Restricted diet and medication failed to relieve the condition to any great extent, however. According to the student's report, he became seclusive, and desperately miserable. He finally sought aid in overcoming an 'inferiority complex.' He could recall no previous history of digestive disorders, except very occasional and mild attacks of indigestion. He had never been socially popular. He was backward and withdrawing, but had never before definitely avoided people. He had often experienced feelings of self-consciousness and embarrassment, but not of utter helplessness.

The analysis of his problem may be summarized somewhat as follows: an individual who steps in front of a rapidly approaching automobile, is not regarded as abnormal, if he jumps back violently, trembles somewhat, shows a pallid face, speaks incoherently or with difficulty, and reports that his digestive processes have been somewhat disturbed. These reactions are characteristic of the ordinary emotional or disorganized response. Should he exhibit the same forms of behavior, whenever in the future he happens to step in front

of an approaching automobile, he is still not regarded as neurotic. Although this behavior may be characterized as disorganized or emotional, and poorly adapted and ineffectual in a social situation, it is usually considered relevant and normal, in response to such an emergency situation. Thus, not every emotional or disintegrated response is to be considered neurotic.

However, the experience of our patient did not stop here. His total disorganized response (hypertension, disturbed digestion, inadequately verbalized anger, and inferiority reactions) was conditioned to the 'kidding' of his companions, to the sight of them, and finally to his persistent implicit dramatizations of 'beating up on them' and annihilating their razzing and kidding with some good retorts of his own. Here we see the establishment of the neurotic vicious circle. The organism becomes disorganized in response to some critical situation. The total disorganization is conditioned to some one or more irrelevant (usually social) aspects of the situation. As long as an adequate combination of these irrelevant situational-aspects are present, they will continue to set off the total response. The visceral malfunctioning may become habituated, in response to persistent dramatization of (implicit reaction to) the situation, and thereby maintain the general hypertension of the organism. This, in turn, becomes the stimulus to further visceral malfunctioning, and so on. Thus, the neurotic response is perpetuated by the conditioning to increasingly remote and irrelevant (and perhaps more numerous) stimuli.

The more complicated forms of symbolic behavior such as language responses (an individual's spoken or written behavior) may become disorganized and conditioned to irrelevant stimuli, as we have seen in the above case. A study of the writings of mystic poets, idealistic philosophers, psychoanalysts, and of the case histories of psychoneurotic patients, soon reveals the extent to which language responses may become dissociated from objective reality. Much of what the psychoanalysts interpret as repression of ideas into the unconscious may be explained by the objective psychologists on

the basis of learning 'not to talk about such things.' Analysis is thus interpreted, not as a method for releasing ideas from the unconscious, but as a technique for directly retraining an individual's verbal responses, and incidentally such other responses as may be associated with the verbal retraining. In response to the questions of the clinician, the patient establishes the habit of explicitly verbalizing those reactions which before had not been verbalized satisfactorily. In the case of the student just mentioned, his social adjustment gradually improved as he learned to verbalize his experience in terms less violent than those which he had used in private and in the earlier clinical interviews. His neurotic symptoms simultaneously gave way to an improved personal integration as he learned to regard his reaction to the critical situation as normal, rather than as a response characteristic of an inferior person.

Once a report of the neurotic patient's spoken, written, and significant overt muscular behavior has been obtained, there is one principle on which psycho-clinicians of all schools agree. The neurotic patient must be brought to face reality. Here we may well pause to determine for ourselves, what is reality! Does reality consist of the metaphysical realm of the psychoanalyst or of the physical world of people, objects, situations and reactions? The objective psychologist believes that a retraining program based on the latter conception of reality is the more effective method of enabling the patient to establish his identity in the physical universe. It should be pointed out that both the objective psychologist and the psychoanalyst use the same methods for studying this problem. They both observe and make a detailed report of the patient's spoken words, written words, and his gestures and postures. Both observe and record his behavior. When such a record has been obtained, we know no more about the patient's hypothetical subconscious, ego, id, super-ego or censor, than we know whether neurons engage in a series of conflicts. We might surely expect such neural "battles" to result in alarming consequences for the behavior of the organism, just as the speculative constructions of the psycho-

analysts are supposed to disrupt it! There seems to be no scientific foundation, however, for advancing a theory of abnormal behavior based either on such an anthropomorphic conception of nerve activity, or on the even more thrilling drama of the psychic mechanisms.

The point is that from observation of behavior-disorganization the psychoanalyst postulates internal (psychic) mechanisms. In terms of these mechanisms he explains 'mental conflict,' which is evidenced only by the behavior-disorganizations on the basis of which he originally postulated the psychic mechanisms. The objective psychologist finds that it adds nothing to his understanding of the problem to speak in terms of such gratuitous postulates. To the latter, the term 'mental conflict' simply means that the clinician has noted a persistently disorganized or inadequate form of behavior.

IV. Social Criteria of Neurotic Behavior

Neurotic behavior has been characterized in this paper as 'disorganized' or 'disintegrated.' At first glance, this may appear to contradict the psychoanalytic conception of neurosis as being a highly organized complex or system of emotionally toned ideas. We are forced to rely here upon the social criterion. We have already seen that the fundamental characteristic of neurotic behavior is its inadequacy in the social situation. Neurotic behavior is *disorganized* from the point of view of effective social and economic participation. Since adequate adjustment to the social environment is the final criterion, we may quite consistently say that non-adjustive behavior is disorganized, and is all the more disorganized when it has been systematized about some remote, irrelevant stimulus. After all, when we say that neurosis is a conditioned disorganization, we are implying in the word *conditioned* that the behavior is to some degree systematized, *i.e.*, that it may be repeated in response to certain specific stimuli or stimulus situations.

It must not be understood by the term social criterion that an absolute and unchanging standard is meant. The

relevance or normality of any form of behavior is a matter of social relativity. It changes from one individual to another, from one situation to another, from one social group to another, from one age group to another, and so on. For example, as young children, many individuals are permitted to develop temper tantrums, headaches, priggishness, emotional dependencies, and other socially unprofitable forms of behavior in response to certain situations, without any particular notice being taken of them. Yet when these same forms of behavior are exhibited by the same individual at a sufficiently advanced age to become socially conspicuous, the individual is considered to be a fit subject for a behavior clinic, a sanitarium, or in more extreme cases, a hospital for the insane. The earlier learning stages in the development of neurotic behavior are seldom recognized at their true significance. When the disorganization reaches such extremes that it inconveniences the family or the neighborhood, the individual is finally regarded as abnormal—that is, no adequate cause can be advanced as an explanation of his displeasing behavior.

Observation of individuals in the early learning stages of actual neurosis convince the objective psychologist that the mechanism of the functional neuroses is essentially identical with that of the 'compulsive' gesture described above, with the exception that actually neurotic behavior is usually, if not always, conditioned to other human beings, rather than to inanimate objects. The mere fact that, in socially stimulated reactions, the exact form and nature of the stimuli are more difficult to isolate and verify than are electric shocks does not in the least invalidate this observation. In accord with this point of view, a functional neurosis may be defined as a learned or conditioned disorganization of behavior which is inappropriate to the situation in which it occurs. An individual does not become maladjusted as the result of neurosis. His maladaptive behavior *is* the neurosis. Thus, in terms of the social criterion, *a functional neurosis is a disorganization of behavior conditioned to, or redintegrated by, a socially irrelevant stimulus.*

This conception takes into account everything which the older psychoanalytic definitions sought to describe, and, in addition, allows for a systematic approach to the problem in the early learning stages where symptomatic behavior may be recognized for what it is. Consequently, the more deeply habituated, and more highly involved forms of disorganized behavior may also be recognized, in the last analysis, as responses to specific environmental situations, rather than as dynamic fulfillments of any psychoanalytic ritual. This conception of neurosis takes the problem out of the realm of mere metaphysical speculation, and places it where it belongs —in the experimental laboratory, along with other problems of human learning.

[MS. received February 3, 1934]

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